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NUMBER I

ATTEMPT AT A DISSERTATION ON THE DISEASES OF PLANTS

BY

JOHANN CHRISTIAN FABRICIUS

1774

A Translation by

MRS. MARGARET KØLPIN RAVN

OF

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With Introduction, Notes and Bibliography by

ERNST GRAM

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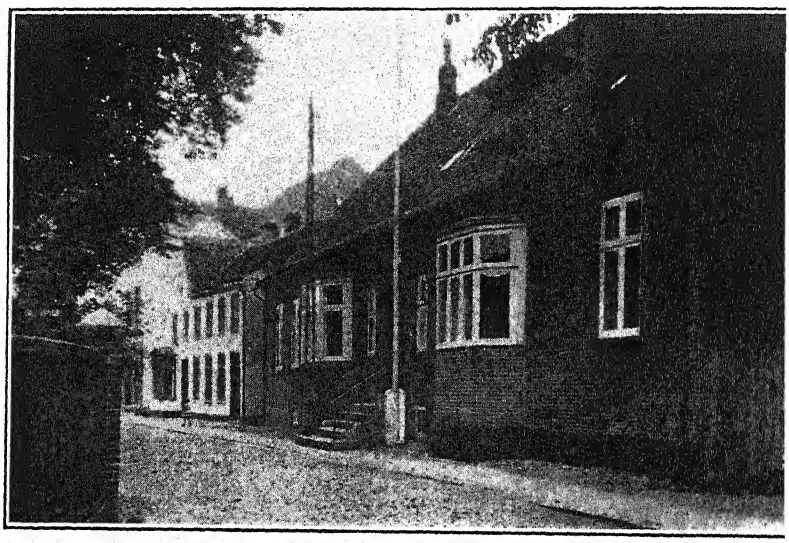

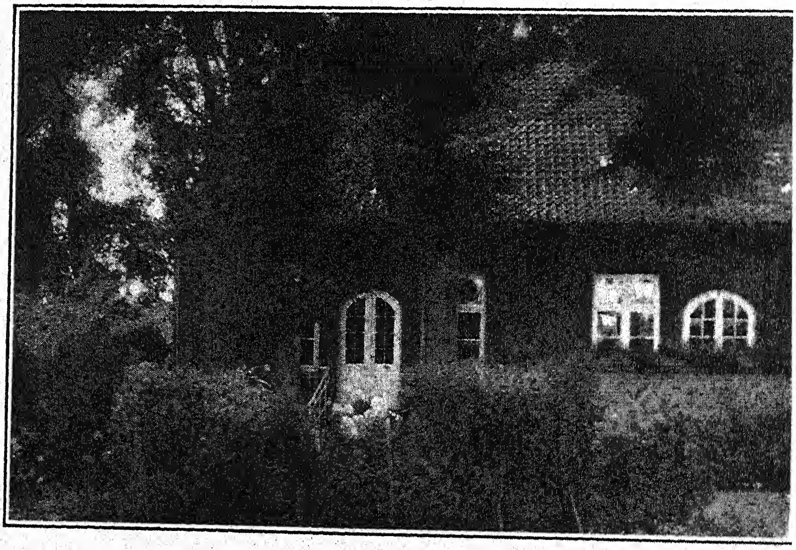

THE BIRTH PLACE OF FABRICIUS

These photographs, showing the street view and the garden of the house in which Fabricius was born January 7, 1745, were taken by Ernst Gram. In his letter of October 2, 1926, accompanying the photographs, he writes: "During a visit to Tønder, the birth place of Fabricius, I succeeded in locating the very house where his father lived and in which the babe J. C. was born. The house is well kept but otherwise as it was originally. Over the door a carved stone reads:

Jeremias XXIX. 5–7
Ann.
MDCCXLII.

INTRODUCTION

By Ernst Gram*

BORN on the 7th of January, 1745, in Tønder in the marshy district of western Schleswig, as son of a physician, Johann Christian Fabricius already in his boyhood surveyed surrounding nature with the works of *Linné* in his hand.

After high school years in Altona we find him studying, 1762–64, with Linné in Upsala, together with his relative Johan Zoëga. Of these two students Linné has left us the following testimony: "Quando Dominus "Fabricius ad me venit cum insecto et D. Zoëga cum musco, tunc ego pileum "detraho et dico: "Magistri mei estote vos" (When Mr. Fabricius turns up with an insect and Mr. Zoëga with a moss, I take my hat off saying: You are my masters). That the feelings of Fabricius towards his master were equally devoted, his quotations and letters bear sufficient witness. In the years 1765–69 Fabricius travelled through Germany, Holland, England, France, and Italy, study-

* Director of the Danish Phytopathological Station, Lyngby.

ing collections and establishing close friendships with the scientific leaders of the century. While abroad, in 1768, he was appointed professor extraordinary at the Royal Botanical Garden and the University of Copenhagen. Seven years later he was summoned to the chair of Science and Economy in Kiel, at this time still a part of Denmark. The University was small, his salary low, and the working facilities poor. Thus, in 1796 (1), he gives a logical plan of experiments, which he would have carried out—with varieties of grains and potatoes, with the influence of manure and artificial fertilizers on yield and quality, with dates of sowing, fodder crops, and cross breeding —*if* he had possessed an experimental field. Most of the year he therefore spent in extended travel, studying in the scientific centers and in the open, and writing "Travels," apparently in order to obtain funds to continue his travels. Every year he passed several months in Paris, and when his advanced ideas of evolution are considered it is apparent that in the breach with the Linnéan system of Creation, in the struggles then going on in Paris, he contributed essentially to Lamarckism and was stimulated as well.

The "Attempt at a Dissertation on the Diseases of Plants" published in 1774 in Det kongelige Norske Videnskabers Selskabs Skrifter (Acts of the R. Norwegian Academy) vol. 5, pages 431–492

(printed in Copenhagen), is an essential contribution to the early development of Phytopathology. By the elaborate system, natural to the pupil of Linné, by the copious material collected, but mostly by the critical remarks, the Attempt far exceeds its sources and contemporary writings. The combined interest of science and economics, an essential requirement for phytopathologists, a great gift of observation, and a keen power of criticism are revealed in the pages of the Attempt.

However, the ideas taken up here were crowded out by his pursuit of evolution and in particular by entomology.

His work along these lines is described fully by *C. Christensen* (4) and *K. L. Henriksen* (7).

With his great scientific gifts Fabricius combined a lovable, helpful and modest nature. He died during a visit in Copenhagen in the year 1808, his illness being seriously aggravated, it is said, from grief over the Danish disaster in the war.

Attempt at a Dissertation on the Diseases of Plants*

By

JOH. CHRIST. FABRICIUS

Translated by

MRS. MARGARET KØLPIN RAVN

KNOWLEDGE of the diseases both of animals and plants forms an important part of our rural economy, but is still too much neglected. We see our cattle fall and our plants wither away without being able to render them assistance, lacking as we do understanding of their condition. Doctors have disdained to condescend to dumb brutes and have delegated them to the mercies of horse-doctors and quacksalvers who are accustomed to use certain mixtures for all diseases without insight and without method. They are acquainted neither with the construction of the body nor the effects of cures, and are therefore unable to adapt or modify the treatment to the conditions or to the observations of the causes of

* A new Danish edition, with an introduction by Ernst Gram, appeared in Tidsskrift for Planteveal 32: 122–155, 1926.

disease. That is why our knowledge of the diseases of cattle is still very incomplete and limited. It is true that of late this science has been studied with great eagerness. France had founded her *Ecole Veterinaire* and other nations have sent thither their clever men that they might learn there the diseases of animals and their cure.

At present however, we remain in expectation of the many improvements to result therefrom, and which we at least hope for and desire.

With plants the condition is far worse; rural economy contains no complete description of their diseases. To be sure from time to time agricultural publications note the best known, though totally without system, and practically no two writers agree as to the causes of these diseases. Many have attributed the damaged growth of their plants to the earth, the sky and the unsound air; this common refuge for the ignorance of doctors has not been forgotten in the case of plants. However the diseases of plants like those of animals seem to be due to internal causes, though indeed I recognize that the conditions of the air can sometimes aggravate the same.

I will therefore essay in this attempt to treat of the diseases of plants and their causes, briefly but systematically.

It is only an attempt, and far from complete, but I will be glad indeed, if I can encourage others

to make closer observations on this so important part of agriculture.

The classification of these various diseases is arbitrary; several could be made and for each could be found arguments for and against. Hitherto attention has been directed only to that part of the plant on which the disease is observed, yet the same disease can attack various parts of the plant; other diseases appear sometimes on one part sometimes on another, while the injury or cause of the disease lies hidden in quite other parts. I will therefore attempt a new method and deduce the classes and genera according to the apparent cases, but the species according to the causes of the disease.

Class I. *Rendering unproductive* are those cases in which the plants are prevented from setting fruit or are rendered distinctly less fruitful. This is frequently not observed until after blossoming.

Class II. *Wasting* are those cases in which the plants are slowly killed, their growth and strength gradually decreasing.

Class III. *Decaying* are those cases in which the main parts of the plants are decomposed and become a rotting mass, which by degrees is transformed to mouldy soil.

Class IV. *Discharging* are those cases showing an abnormal flow.

Class V. *Rendering misshapen,* recognized by the abnormal development of the external parts.

Class VI. *Extraneous* are those cases due to the apparent injury to the parts.

CLASS I

Rendering Unproductive

GENUS I. Super-abundance of sap.
Polysarkia

Super-abundance of sap is really an indication of great health in both animals and plants; however in the latter it sometimes occasions an unfertility, wherefore it must be grouped among the diseases. Too abundant sap is easily recognized from the great number of shoots and leaves, while of blossoms and fruit the plant sets only a few or none at all. This is very often observed on fruit trees in the gardens of those plant lovers who, disregarding the nature of their trees, believe that the soil can never be rich enough, and the disappointment to their hopes is the more bitter in that it was at first nourished by the luxuriant growth of the trees. This disease, too, is sometimes found on cereals, though seldom, and only on sturdy

plants, which, though too abundant fertilization has encouraged the growth of straw and head, yet yield a smaller number of seeds. The cause of this disease is to be found in the too abundant flow of the life-giving sap which renders wood and bark too strong. The internal, marrow-like part of the plant, which is sundered at blossoming, is unable to combat the opposition of the sturdy external parts, and therefore unable to develop into flowers and fruit. (*Cf. Linn. Dissert. Prolepsis Plantarum.*) This is why well-developed branches never bear fruit, while the weaker ones do so constantly, and why gardeners set those plants which they desire to produce blossoms in small pots in a warm place. The scanty nourishment allowed hinders the development of the external parts, whereas the warmth stimulates the development of the internal parts, enabling them the more easily to sunder the external parts and blossom forth.

We have various remedies for this disease. It is possible to reduce the resulting unfertility by extensive pruning of the woody parts; however this does not remove the cause of disease. Another remedy is to replant or to place other soil about the roots, or again, to remove a section of the large vertical tap-root, thus diminishing the supply of life-giving sap.

Genus II. Smut. *Ustulago*

Smut on cereals is one of the most common and best known diseases; it often nullifies all hope of a good harvest. It attacks by preference wheat, barley and oats, while rye and millet seldom or never suffer. Eradicating this disease has become the more difficult after recent careful investigations have proved it contagious. When it has once gained the upper hand in a field, it increases in strength year by year. We are indeed unable to cleanse our fields of it unless we resow them with clean, fresh seed. This disease is still to be found on several of our wild grasses, especially *Panicum miliaceum* (Schwaden), *Triticum repens,* cock's-foot grass, and a few others.

Smut only attacks the floral structures which it gradually transforms to a loose black powder easily blown about by the wind. While the plant is germinating and growing no traces of the disease are observed, but as soon as it heads, smut attacks the tender parts of the kernel and changes them to that black powder, especially noticeable when the grain is threshed, for it flies about everywhere. Moreover the disease so substantially decreases the number of the kernels that in many fields more smutted than healthy grains are found.

Much care has been expended in a closer study of the causes of so destructive a disease, hoping

thus to find means for its prevention; hitherto no consensus of opinion has been reached. Tillet claims, in a paper which won the prize offered by *l'Academie Francaise* for this subject, that smut is spread by contagion alone. In this he only upholds our own opinions in regard to its contagious qualities without showing the real causes. If we were told that contagion was the cause of cattle disease, would we be expected to believe that a new and important discovery had been made? Every farmer knows that cattle disease and smut both spread because they are contagious; what we wish to know are the actual reasons for the cases of diseased cattle and smutted grain.

Professor Gleditsch, on the other hand, attempts, in an extensive and diffuse treatise, included in his physical botanical papers, to prove that those kernels which have not attained full maturity at harvest time, undergo, in their place of storage, a kind of fermentation, from which later, when the seed is sown, smut is derived. He quotes some examples which seem to prove his point, yet even so, much doubt remains. How can it be that it is contagious when true smutted heads yield no kernels able to spread fermentation? How can it be that no smut is observed the first season after fresh seed has been sown? We may hardly assume that the kernels attain greater maturity during these seasons. How can it be that only a few

species of plants are attacked by smut even though undeveloped seed are to be found in all. Rye, millet, and many other cereals and grasses are very seldom or never attacked by smut even though the bad weather of autumn obliges us to reap early. Should seed which has not ripened properly, and therefore undergone a kind of fermentation, not give the slightest sign of injury during its development until it heads? This is hardly to be expected of half-spoiled seed.

Herr von Mynchhausen in the first part of his "Advice to Householders," and von Linné (Knight) in a *Dissert. Mundis invisibilis,* claim that when the black smut powder has soaked in water for some days it is dissolved into small worms, and these they believe are the true cause of smut. A kind of movement is always observable when the black powder has been saturated; whether this is due to something animal, to something organic, or whether indeed it is the cause and not the effect of the smut, is not absolutely certain. However, certain it is that the cases and symptoms of smut can never be better explained than by assuming something organized to be the cause.

Many authors describe still another kind of smut, the so-called stone-smut, but with this I am not sufficiently acquainted. Cereals attacked by

this are said to become black, hard and shiny. This would seem to indicate that it is a species of the real smut.

Two remedies are used to prevent smut on our fields: sowing pure seed, and soaking the seed in a solution of salt or lime. Both remedies are highly praised and indicate, moreover, that the cause of smut is not to be sought in the soil but in the seed.

Genus III. Barrenness. *Aurigo*

Barrenness is that disease of plants due to which, even though the plants blossom, they, after blossoming, show not the slightest sign of setting fruit. We observe the phenomenon annually and frequently, both in our fields of cereals and in our gardens and orchards. Sometimes our plants suffer more from this than from smut, though it is less conspicuous. The straw grows well and abundantly, but the heads point directly upwards, indicating that they are lighter than the normal heads and therefore contain less kernel. Sometimes the entire head is attacked by the disease and it then consists of empty hulls; sometimes we find empty hulls and full hulls side by side, but we seldom find a head in which every hull contains a kernel.

The common cause of this disease lies in inhibited fructification. The different sexes of plants

have already been sufficiently demonstrated, but it is there we must seek for the cause of barrenness. If the pollen of the male flower is entirely lacking, or rendered ineffective, or prevented from falling on the moist *stigma* of the female flower, no fructification occurs, the blossom falls off without showing the slightest sign of fruit. We find many species of this disease, because the causes preventing fructification differ.

1st Species: *Barrenness due to rain.* Too persistent rainfall is the usual cause of barrenness in our Northern countries. We lie low for the most part, surrounded by high mountains, therefore the weather here is, as a rule, variable and damp. If during blossom-time constant rain sets in, the rain makes the pollen moist, heavy, ill-adapted to flying about and so to fructification.

2nd Species: *Barrenness due to cold.* Our Northern countries are also frequently subject to this disease. Actual winter cold very seldom or never inflicts suffering on plants, for at that season all growth ceases entirely and the cold is seldom severe enough to split the trees asunder. However in the spring, and sometimes far into the year, the incipient warm weather is often broken by frosty nights from which the plants suffer the more. During the months of March and April we often see our fruit trees and other plants full of blossoms and filling us with the sweetest

hopes for a rich harvest; a single frosty night and the hopes are utterly destroyed. In barrenness due to rain the seat of disease is the pollen, in the present case it is to be found in the parts of the female flowers, for when cold has caused the moisture-filled vessels of the stigma to expand, they explode and are thus rendered unfit for fructification.

3rd Species: *Barrenness due to smoke.* Smoke is very detrimental to the fructification of plants, for it dries up the liquid in the stigma necessary to fructification. I once observed a splendid example of this disease in Germany in a wheat field. In one end of the field some tramps had camped out while the corn was in blossom. The field was beautiful, with large, full heads, only in the path where the smoke had drifted across were the heads empty.

4th Species: *Barrenness of bastardized plants.* Bastardized plants is the name we give to those which spring from the crossing between two different plants; they are the same among plants, as are mules among animals. They are not propagated by seed, yet occasionally by root as *Peloria Linnaei.* The cause of their unproductiveness seems to lie partly in the lack and partly in the consistency of the pollen of the male flowers. See Koelreuter's "Experiments and Observations on the Sex of Plants."

5th Species: *Barrenness due to lack of male flowers* is to be found in part among the so-called dioecious plants (*plantis dioicis*), of which the immortal von Linné (Knight) gives many examples in proof of the sex of plants. The female flowers blossom perfectly, but they wither without the slightest indication of fruit if no male flowers are to be found in their vicinity to fertilize them. This disease is also to be found among monoecious plants (*plantis monoicis*), in gardens, melons for instance, cucumbers and others from which gardeners, under too thorough pruning, remove the male flowers, thus preventing the fertilization of the female flowers.

6th Species: *Barrenness due to lack of motion.* This is found more often than one might believe. Plants enclosed in greenhouses often bear no seed, and the above seems to me the reason for that kind of barrenness. A certain amount of circulation of air is necessary, especially for *Dioicis,* to scatter the pollen and the greater the force with which the pollen falls on the *stigma,* the more complete is the fertilization. Insects, bees and others which work in the flowers are often substitutes for this lack of motion and I do not in the least doubt but that they are absolutely necessary for the fertilization of those plants in which the perianth closely covers the organs of fertilization.

GENUS IV. Unseasonable Deciduity. *Abortus*

Unseasonable deciduity is the name we give to that disease of plants under which, even though the flowers are fertilized, the fruit withers and falls off before reaching maturity. We find many examples on cereals in which the kernels are set but dry up in their hulls. The straw develops well but the heads are light, and when the hull is opened only a small unripe kernel is to be found containing no flower. Fruit-trees in orchards are likewise subject to this disease; the fruit that is set dries up without apparent external injury and falls off. This disease does not hinder the growth of the trees but the fruit of that summer is lost.

The cause of deciduity lies either in the lack of nutritive sap or in the prevention of its flow. The plant is unable to nourish the fruit set; this gradually dries up and falls off. As many circumstances contribute to this lack of nutritive sap or to its lack of effectiveness, many species of the disease are observed.

1st Species: *Deciduity on young plants.* The disease is very prevalent on young and growing trees; these blossom abundantly but are incapable of bringing all the fruit set to maturity, and therefore allow them to fall off unripe. The more abundantly a tree blossoms, the greater is the drain on the nutritive sap and the smaller the quantity of fruit reaching maturity. Careful

gardeners, then, remove some of the blossoms and thus secure a greater quantity of fully developed fruit, and also thus prevent the young trees from exhaustion.

2nd Species: *Deciduity due to drought.* A severe and long-continued drought greatly diminishes the moisture in fields, and the plant nourishment actually present in the fields is prevented from penetrating into their fine vessels. The straw of cereals becomes yellowish, the heads light; when the hulls are opened, undeveloped kernels are found.

On grass varieties and mosses which wither completely during dry weather and freshen again under the slightest rainfall, this disease is very apparent. Large plants on the contrary, with roots deep in the soil, do not so readily risk deciduity due to want of moisture, for, penetrating as they do so much deeper into the earth, moisture is not lacking.

3rd Species: *Deciduity due to cold.* If a heavy frost occurs after the fruit is set, the chill splits the vessels swollen with moisture which are to lead the nourishment to the fruit, and the fruit rots and falls off. This disease is often observed on fruit trees, which, for the sake of warmth, are planted near walls, for they always blossom and set fruit early.

4th Species: *Deciduity due to lack of nourishment.* This form is often observed on our sandy, unfertile fields. They are sown annually but the fertility found in the soil is insufficient to bring the kernels to maturity.

5th Species: *Deciduity due to injury of the stalk.* When the straw of cereals is broken by external injury, though not entirely torn away, the vessels are so compressed that the sap can no longer flow in as great quantities as before and the grain already set diminishes and fails to attain the necessary degree of maturity and perfection. This is why heavy gales, violent rain and hail do so much damage to our fields of cereals.

6th Species: *Deciduity due to the sting of insects.* Our plants suffer most from the sting of insects. These we do not so often see because they are so small, however their almost countless hordes and turbulent fecundity quite compensate for their small size. They are often the cause of the above disease. They bite a hole in the fruit and suck nourishment, finally causing the fruit to dry up. We can often see such dry fruit hanging on trees and for this, insects only are to blame. The Knight Linné computes the damage done by *Oscinis frit* on barley alone at a very large sum.

Genus V. Doubling. *Plenitudo*

Doubling of flowers occurs when the perianths increase, thereby preventing the formation of the

internal parts essential to the flowers. In this way the stamens are either pushed entirely aside and the flowers become barren, as fertilization cannot occur or the perianths by their increase push the stamens partly aside, in which cases fertilization occurs but occasionally. The care taken under forcing and the super-abundance of nutritive sap are the real cause of double flowers. Our horticulturists use all their arts in their attempts to produce these and to make them ever larger and more beautiful. This is the triumph of horticultural skill, as the high price asked for these double flowers and their magnificent but not sufficiently characteristic names, indicate. Sometimes, but indeed seldom, we observe doubling on fruit trees and on wild plants which is our reason for grouping it under plant diseases.

1st Species: *Doubling due to increase of the external perianths.* This form of doubling is rarely seen, nor does it seem to injure the fertility of the plants.

2nd Species: *Doubling due to the increase of the internal perianths.* This is the most common form of disease. Roses, hyacinths and anemones present many cases in which, due to the increase of the petals, the stamens are entirely shut out and therefore barren. Other flowers, the monopetalous, *Flores monopetali,* for instance, double only the internal perianths without excluding the

stamens and therefore often run to seed. Flower specialists designate the former group, "double flowers," (*Flores pleni*), the latter, "enlarged flowers," (*Flores multiplicati*). Finally another form is found in which a new stalk grows from the double flowers; this develops, forming partly leaves, partly flowers. This sometimes occurs on roses and also in the double daisy, (Bellis), and is known as "proliferate flowers," (*Flores proliferi*).

3rd Species: *Doubling due to the increase of the honey vessels,* (*Nectaria*). This form has only been observed on a few plants. Sir Linné, in his *Philosophia botanica,* page 83, notes this form of doubling on *Nigella* and *Aquilegia,* where sometimes the increase of the honey-vessels shuts out the internal perianth, and sometimes the perianth remains, the honey-vessels, however, increasing in size and uniting between the petals.

4th Species: *Doubling due to the enlargement of the internal perianths.* This form is observed on many plants which bear two kinds of flowers, *viz.*: fertile, with a very small perianth inclosed by others which are barren, *Flores composita* (composite flowers). *Viburnum Opulus* and others are examples in which doubling consists in the internal, fertile flowers assuming the form, size and barrenness of the external flowers.

CLASS II

Wasting

GENUS I. Choking. *Suffocatio. Etiolement Adans. Famil. des Plant.*, I., 48 (2)

Choking is recognized from the scrawny, puny appearance of the plants attacked, their long, dry twigs and their scanty yellow leaves; the more this disease gains the ascendancy, the paler the color of the plants becomes, until at last they dry up without bearing fruit. The cause of the disease is to be sought in the lack of circulation of air without which neither plants nor animals can exist. Both need to perspire in pure healthful air, which is in motion since this increases perspiration; this is the reason for the unhealthy condition both of people and plants confined to foggy places as their color indicates. The disease is likewise observed on *Zoophytes,* the connecting link between the animal and the vegetable kingdom. These are destined to grow at a certain depth below the surface of the water, where the external motion of wind and wave cannot penetrate, therefore their flowers are endowed with a spontaneous motion without which they could not exist.

1st Species: *Choking due to confinement of locality.* This form of the disease is often observed on young trees surrounded on all sides by others of tall growth. They grow straight with

long weakly shoots until their tops are on the level with the tops of the surrounding trees and they can then enjoy the effects of the free air. If they have not sufficient strength to attain this height they gradually wither away from sheer lack of air and exercise. Sensible gardeners are cognizant of this disease and seek to prevent it by thinning out among the trees which stand too close.

2nd Species: *Choking due to lack of light.* I do not know what light does to further the growth of plants, but experience proves that it is a factor necessary to their development. For this reason in greenhouses those plants furthest removed from the windows are the weakest, also the plants which are artificially forced during the winter have a pale yellow color, the more they are forced, the paler they are, and the weaker their perfume, and further, plants growing in dense forests, even those native to such places, are always of a sickly, yellow color and never have the bright appearance of other plants.

3rd Species: *Choking due to trailing plants.* Among our noxious species of weeds are some which, with their long slender vines, bind plants together, prevent their motion and completely choke them. To these belong the so-called dodder (*Cuscuta*), and bindweed (*Convolvolus*), which are often a nuisance in gardens, for thick stems and large leaves require free movement.

4th Species: *Choking due to insects.* This, according to the testimony of Sir Linné, the Knight, is the form of the disease most often observed in greenhouses. It originates on *Acouo telario Linnaei,* which draws very tiny, hardly perceptible threads across leaves of plants; these become yellow and finally fall off.

Genus II. Decline. *Tabes. Jaunisse*
Adans. Famille des Plantes, I., 48 (2)

Decline is the form of disease under which plants gradually lose their fresh appearance, their growth decreases and they finally wither away. In the earliest stages of disease, the leaves are limp, later they dry up. The stalk begins to show a yellowish color and at last the whole plant dies. This disease often attacks a part of a plant only, a single branch; and then, as a rule, the tree may be saved if the diseased part is sawed off and destroyed.

The cause of the disease is to be sought in inhibited circulation of the small amount of nutritive sap necessary for nourishing the plant. As soon as a part or the entire plant lacks its required nourishment, it suffers, presents a sickly appearance and finally dies. It may show green leaves for a certain period, but these are, as a rule, covered by a horde of plant lice which soon drain the remaining strength of the leaves.

1st Species: *Decline due to lack of nourishment.* Soil nourishes plants just as long as it contains the necessary nutritive substances. When these are exhausted, either because the plant growth is too dense or because the soil is naturally poor, the roots can no longer take up nourishment and the plants decline and die. Our poor and sandy fields, and the plants growing on scanty soil among stones, present sufficient examples of this disease.

2nd Species: *Decline due to unsuitable soil.* Nature has provided special foods for animals, and diverse soils for plants, in order that one shall not thrust aside nor overwhelm another. If plants are removed from this soil and placed in one unsuited to their character, they are not always able to adapt the juices to their use. They grow weaker each year and are covered by so many plant lice that they are finally totally destroyed and leave no trace whatever of their presence. Many examples of this disease are to be observed in the gardens of the ignorant; moreover the various mixtures which horticulturists use in watering their gardens often cause decline.

3rd Species: *Decline due to injury to the parts.* As soon as those parts which the plants use in absorbing nourishment are injured, decline at once sets in. A typical example is the injury to the roots due to the activity of the root worm so often observed in our meadows.

4th Species: *Decline due to evaporation after transplantation.* Newly transplanted plants are not immediately capable of absorbing as much nourishment as the evaporation in the severe heat of summer requires; we therefore often see them wither after transplantation. This is why skillful gardeners always transplant in cloudy or rainy weather, or at any rate secure shade for the plants.

5th Species: *Decline due to premature shedding of the leaves.* Most of our plants shed their leaves in the autumn, but until that time they are necessary to the trees. If shed sooner, the trees suffer and often die, as those trees bear witness which are deprived of their leaves in the spring by the cockchafers, or the mulberry trees which are too greedily stripped of their leaves to furnish food for silkworms.

6th Species: *Decline due to discharge.* When secretion of the resinous parts is too copious, decline often ensues, because a large part of the nutritive sap has thereby been lost. This form of disease is often observed on young spruce and pine trees after too eager collectors of resin have made too many and too deep incisions in the bark. See Genus I under Class III.

7th Species: *Decline due to bleeding.* Bleeding is a natural characteristic of many plants; however if these watery secretions are too copious,

due to some external injury, decline often results. This form of the disease is often observed on grape-vines pruned at the wrong season, also on birch trees which have been tapped.

8th Species: *Decline due to parasitic plants.* We call those plants parasites which plunge their roots deep into the substance of other plants and draw therefrom nourishment to themselves. Plants suffer much from these parasitic guests. When the latter propagate too abundantly, a decline ensues which can prove mortal for them both.

CLASS III

Genus I. Discharge. *Extravasatio. Depot Adans. Famil. des Plant.*, I., 49 (2)

Discharge consists of the secretion of a slimy liquid through the bark; this thickens as soon as it comes into contact with the air and finally hardens. The sap thus secreted can either be dissolved in water and is then known as gum, or it can be dissolved in various kinds of spirits and is known as resins. These in turn vary according to the different species of trees which have produced them and are used both as medicaments and incense. We seek, therefore, by artificial means to propagate this disease in order to obtain the various secretions in greater amounts. We cut incisions in the bark in the spring, and in the

autumn we gather the coagulated sap. However the growth of the trees always suffers more or less under the treatment; if we cut too many, or too deep incisions, they fall into a decline and wither away. This disease is due to the bursting of the vessels of secretion; in this it may be compared with the various kinds of bloody discharge known in animals, and which, though sometimes beneficial to health, yet always occasion a bodily weakness. Nature expends a large part of her forces in compensation for this injury, which only results in the discharge becoming more and more copious. However it is seldom that the natural secretion of plants is so great that remedies become necessary. The best treatment is to prune away the injured part and bind the sore with tree wax.

1st Species: *Discharge due to injury to the bark.* This disease may appear whenever the bark is injured, whether the injury is due to accident or intention. The greater the injury, the more copious the discharge.

2nd Species: *Discharge due to abundance of sap.* The quantity of juices is sometimes so great that the vessels are unable to contain it all. These enlarge as much as the parts permit, and then burst; the discharge is then apparent.

3rd Species: *Discharge due to the tartness of the sap.* Saps and juices of plants, like those of

animals, are sometimes so pungent that they eat up the vessels of secretion and then overflow. Plants growing near muddy water often have a discharge due to this cause.

Genus II. Lachrymation. *Lacrymatio*

Lachrymation consists of the secretion of a watery fluid from the eyes. This disease is quite unlike the preceding one, for in this the nutritive sap flows out through the eyes or through the vessels of the tree, while in the former the sap belonging to each plant flows out through the vessels of the bark. Therefore they differ both in respect to the place from which the fluid comes, and in the fluid itself. This lachrymation is not observable on all plants. It is characteristic of a few only,—the grape-vine, the birch tree, the maple tree and some willows. It is not always present, but in the spring, when the sap begins to rise, it is most generally observed. It is therefore a general rule among horticulturists that those plants which are subject to lachrymation should not be pruned at all in the spring, or in any case only lightly, thus preventing their being weakened by the ensuing copious discharge. The correct season for pruning is the autumn, or very early in the spring, before the sap has begun to flow. A hole is bored through the bark of birch trees and maple trees and of the palm trees of

India, thus encouraging the discharge, which is known variously as birch water, maple syrup, palm wine. The trees however suffer from the treatment.

1st Species: *Natural lachrymation.* In the spring some trees shed tears (bleed) through their eyes, but this is more an innate characteristic of the variety than an actual disease. This natural lachrymation has no evil after-effects, for it is rarely copious enough to weaken the trees. However the more abundant their nourishment and the more super-abundant their nutritive sap, the more copious the lachrymation. Later, when the leaves develop, this ceases of itself.

2nd Species: *Lachrymation due to external injury.* There is no better way of improving a tree than by judicious pruning. On the other hand if trees are pruned in the spring, or sustain an injury which penetrates to the wood after the sap has begun to ascend, copious lachrymation ensues, the tree is weakened and finally goes into a decline.

Genus III. Honey-dew. *Erysiphe, Le Givre Adans.*, I., 45 (2)

Honey-dew is the secretion of a thick, slimy liquid on the surface of the leaves. Plants attacked by this disease have large shiny spots on the leaves which, when the attack is severe, run

into one another and drip; however they are soon dried up and scorched by the heat of the sun. The attack is generally worse in the cool of the evening, and plants suffering from this disease are often destroyed in one night. They are permanently weakened, bear tasteless fruit or none at all. Honey-dew is the most common and malignant disease of hops. It can destroy an entire hop plantation in a single night. Much pains have been taken to discover the true cause of this pernicious disease,—as yet naturalists differ. Some claim that honey-dew, like other dew, only falls when certain winds prevail, which is why it is called dew; however this opinion is at odds with the nature of dew and with the circumstances attendant on honey-dew. Dew which is common to all animals and plants is never of so harmful a character that it is responsible for the destruction of an entire neighborhood. However honey-dew attacks only certain species of plants, and that, too, not all the plants of the same species. We observe a hop field reeking with honey-dew, yet the adjacent plants are untouched and what is still more surprising, the same is true of certain other plants in that field. The weakened condition of the plants after the attack indicates that some of their sap must have mixed with the honey-dew.

Adanson, in his *Famille des Plantes* (2), gives inhibited evaporation, due to the lack of pure

fresh air, as the cause of honey-dew. We are familiar with other diseases which are caused by lack of circulation and evaporation; however the circumstances attendant on honey-dew differ so greatly from those due to inhibited evaporation that I am unable to concede this as the cause of honey-dew,—especially in view of the fact that the great quantity of sap secreted seems rather to indicate a too abundant than an inhibited evaporation. Others believe that the disease is caused by plant lice and snails which are present in hordes on the diseased plants. This seems to me rather a result than a cause of the disease. Insects and snails find pleasure in the sweetish juice which has been secreted and come from all the adjacent plants to suck it.

Linné, the Knight, in his "Journey to Skaane," gives another and very different cause of the disease. He believes it to be due to the larvae of *Phalenae Humuli,* which attack the roots of hop plants, and he claims to have caused the disease by depositing the eggs of that nightly visitant about the roots of hops. It seems reasonable to believe that the disease is occasioned by injury to the internal parts necessary to the life of the plant. The art of medicine teaches us that those suffering from injury to the internal vital parts lose all their strength in heavy nightly sweats. May not a resemblance be found between such

nightly sweats and honey-dew? Injury to the root occasions poor nutrition with a resulting weakness of the entire plant which at last develops into honey-dew. Yet it is very strange that this honey-dew generally appears in a single night and disappears again just as quickly without a single sign being visible either before or after the attack.

Various remedies have been tried for this disease, depending on the cause to which it is attributed. One in particular is described in the Agricultural Archives, No. XX, p. 712, and I have requested a detailed account of the results obtained (12). This remedy consists in the application of pig manure as fertilizer. We know that pig manure is the most effective remedy for insect pests, and when, as in this case, the remedy is found efficacious, it seems to be a strong proof that insects are the cause of the disease.

CLASS IV

Decaying

Genus I. Rot. *Caries. Pourriture*
Adans., I., 50 (2)

Rot consists of decay of the woody portions of the stem (trunk), and their gradual transformation into mouldy soil. This disease usually begins from without, gradually spreading until the entire tree is hollow, only the bark being left, filled with

a soft, rich mould from the rotten tree. Willows are particularly susceptible to this disease and we often find that nothing remains except the outer bark, yet the shoots annually put forth green leaves. Insects, especially *Dermestes* and *Cerambyx,* contribute much to the destruction of the tree. As soon as a portion of the wood is rotted, they bore into it in all directions, giving the dampness access, and furthering the process of decay. The cause of the disease is the inhibition of the flow of sap which then becomes pungent, attacking and destroying the hard portions of the tree. Every static fluid, whether contained in the nutritive vessels themselves, or penetrating to them from without, has the power of causing such rot.

1st Species: *Rot due to the accumulation of rainfall.* Nature intends the bark to keep out air, moisture and rain. If the outer bark is injured, or if a branch is cut off so that rain water can collect in the wound and act on the woody portions of the trees then this stagnant water, and the pungency it contains, will cause rot. This is why gardeners so carefully treat wounds on trees, and the stumps where branches have been lopped off, with wax, thus preventing the action of air and water.

2nd Species: *Rot due to the stoppage of the nutritive sap.* This sap possesses the same characteristics as the other fluids; when circulation is

prevented it becomes pungent and attacks the adjacent parts. Therefore when the nutritive sap is super-abundant and its flow is checked, the tree soon rots. This is often observed on willows whose branches have been lopped off. The sap which hitherto flowed through these branches cannot immediately readjust itself; it becomes choked and causes rot, even though the trunk is very carefully protected from the air and the internal juices. The only sure method of treating rot is to cut out the injured parts, and, in so far as is possible, with a vertical cut, afterwards sealing the surfaces thoroughly with tree wax to keep out dampness.

Genus II. Putrefaction. *Putredo*

Putrefaction is the decomposition and decay of the soft parts which are gradually transformed to a slimy, muddy substance. This disease either attacks the roots, and in that case the entire plant dies at once, as its nutrition is cut off, or it attacks unripe fruits, as we often observe in fleshy fruits and cereals.

The cause lies presumably in a super-abundant and suspended fluid which attacks the adjacent parts until they ferment and become mouldy. External conditions, damp weather for instance, add their contribution, and in wet seasons we are witness to the way in which mould spreads over our fields and orchards.

1st Species: *Putrefaction due to too rich soil.* All plants require their own special kind of soil; if we give them one which is richer or fatter than the one where they are accustomed to grow, they often suffer from this disease. The sap flows in greater quantities than is necessary for nourishing the plants, it is therefore stopped in the various vessels, becomes pungent and destroys them. This is often experienced by flower lovers who plant carnations and set their bulbs in soil which is too well fertilized, in the hope of attaining larger and more beautiful blossoms, but of these the larger parts moulder away.

2nd Species: *Putrefaction due to too much fluid.* Any suspended fluid, either sap or rainfall, causes this disease. It is observed on plants in flower-pots with no holes in the bottom through which the water can be drained off but which, nevertheless, are daily watered thoroughly. The soil becomes sour, moss grows on the surface and the plants rot. Cereals sown in the field suffer from dampness, especially in our climate, when sown too late in the autumn, after the wet weather has set in.

This disease is found on soft fruits, especially cherries, and on certain of our cereals too, for that matter. Super-abundant moisture causes them to mould on the stem before attaining maturity.

3rd Species: *Putrefaction due to external injury.* When the external parts, root or fruit, are

injured, moisture gains admittance the more easily and causes the disease.

4th Species: *Putrefaction due to parasites, cited by Duhamel in Act. Parisiens. on saffron* (5). In the same article he describes a fungous growth which attacks the knobby roots of saffron, changing them to a mouldy, rotting mass.

Genus III. Canker. *Cancer*
Adans., I., 52 (2)

Canker is characterized by large protuberances or growths on the trunk (stem) which constantly, even at the dryest time of the year, exude a corroding juice which attacks the adjacent parts. Trees often suffer very much from this disease, and I have seen gardens, especially in our low, damp marshes, where almost all the trees were attacked. They wither, and finally, when the disease has penetrated to the internal parts, dry up entirely. The cause of the disease is to be sought in deranged nutritive sap which in the end becomes pungent and rotten and eats into the adjacent parts. The more rotten the sap, the more quickly and completely does it attack the tree.

I do not know more than one variety of this disease due to rotten, putrid water. The poison of the water enters the nutritive sap, which, in turn, gradually eats into the vessels and causes the disease. A few years ago I saw with my own

eyes a quince tree planted in a low place into which an adjacent manure heap drained. This caused a very severe attack of canker on the tree, though no other plant in the entire garden was affected by the disease.

Adanson (2), in his pamphlet referred to above, tells of another species of this disease, namely, one in which the noxious secretion does not exude, but remains between the wood and bark. Even the strongest trees wither when attacked, without showing the slightest indication of any secretion of pungent juices. They retain their dry, withered leaves and when hewn, the noxious secretion is found between the wood and the bark.

The only sure remedy for the disease is to cut out the entire protuberance. In this way the noxious secretion is withdrawn. However the cause of deranged nutritive sap must also be removed, otherwise the disease will soon return.

GENUS IV. Rust. *Rubico. Rouille*

Duhamel: Elem. d'Agriculture, Tom. I (6)

Rust appears both on the stem and on the leaves of plants; it rends the tender covering and hides the surface under a brownish and light powder. Often it only occasions small spots on the leaves and affects the development of the plant but slightly. However, when it gains the upper hand

and attacks the stem, the plant suffers greatly; sometimes dries out entirely.

Rust is found on many plants, especially on wheat and rye in the fields; the straw splits and pours out the brown powder. In our northern climes the disease is less common and of lesser importance than in Southern Europe, where it often totally destroys all hope of a rich harvest.

Rust seems to me to bear much resemblance to smut. Smut transforms the organs of fructification to a black powder; rust transforms the parts below the epidermis or outer covering to a brown powder,—the greatest difference between the two cases appears to lie in the place in which they are found. At all events I believe that the cause of both these diseases is one and the same. The author of a small paper on rust on cereals attributes this to insects, although his paper contains no proof of his theory. Insects often cause very material injury, yet because these insects are so tiny they are not always observed; however this is no reason for denying the injury. Mind and imagination are captivated by small and great alike, for in each, nature is infinite. Meanwhile more time and the diligent research of our successors are necessary to discover the cause of both these diseases.

As remedies for rust the farmers suggest the same as used for smut,—a change of seed and

soaking the seed in a salt or lime solution prior to sowing.

CLASS V

Injury

GENUS I. Fissures. *Fissura. Jerses Adans.*, I., 45 (2)

Fissures mean that either the outer bark of the tree, or the internal wood itself is split lengthwise. The first class is easily recognized as the damage itself is visible, the second, in connection with which we often hear a strong rustling in the uneven bulges of the bark is due to the difference in the length of the wood at the point of injury. Neither the fissures of the wood nor of the bark ever heal but grow longer and longer and render the wood unsuitable to carpentry.

The cause of fissures is an enlargement of the internal parts to such an extent that the external cover can no longer protect them, but splits. We often see the disease on young trees of rapid growth whose vessels are swollen by abundant moisture.

1st Species: *Fissures due to cold.* Cold as well as heat expands bodies; therefore after a very severe winter our trees split lengthwise with a loud report. However this disease is not very common and the frost must be very severe before

it causes the injury. In gardens in which foreign varieties of trees are exposed to a certain degree of cold we attempt to prevent the disease by placing manure about the roots or binding straw about the trunk.

2nd Species: *Fissures due to abundance of sap.* Too great flow or super-abundance of sap may also cause fissures on trees, especially when protracted hot weather has expanded the juices. It is true that due to the flow of sap the bark becomes more pliable and stretches very considerably, but on trees growing on soil that is too rich this expansion is not sufficient to prevent the disease and the bark and wood finally split. The only way to prevent the disease is to remove the super-abundance of sap which is its cause.

Genus II. Galls. *Gall*
Adans., I., 47 (2)

Galls is the name applied to knobs and excrescences due to the stings of various insects. We find them on all parts of plants, the only differences being their appearance, size and location. They often attain considerable magnitude and assume strange forms which attract the eye of the scientist. We see for instance so-called gall-apples on oaks, while the Bedeguar on rose bushes and the large knots, or Tophi, on the trunks of

trees seem also, to have insects to thank for their presence.

The sting of insects is, then, the cause of galls; they lay their eggs in a wound located somewhere on the plant; this wound cannot heal as long as it contains a foreign substance. The maggots creeping out finally devour the adjacent vessels and suck nourishment from their nutritive sap whose flow is increased, nature wishing to drive out what it extraneous. At last the nutritive sap increases in these unnatural protuberances which continue to grow as long as the maggots gnaw the adjacent vessels of secretion. *Linn. Cynips* is the actual noxious insect, and each species causes a different gall-form.

The so-called ergot (Clavus) is no other than a form of this disease. It comes about in a similar way: *Thrips Physapus Linn.* attacking the tender rye kernels and laying its eggs in them, the kernels develop over the husk, become blackish, lumpy, internally fungous and containing a rotten, brownish powder. The same is observed on the flowers of *Lotus corniculatus* and *Cerastium,* but on no other cereals, rye alone excepted. Many doctors have claimed that this ergot is the cause of cattle-disease (*Raphania Linnaei*); however experience proves that this is not so. In various districts of Holstein, children hunt out these kernels and eat them in great quantities without ever

being attacked by this disease, and I am sure that no one can consume such quantities in bread as these children do when eating them alone. The latest report from physicians seems to admit the same.

GENUS III. Monstrosities. *Monstrositas*

Monstrosity is the name given to the abnormal growth of plants or of a part of a plant. It is often due to the manner of cultivation, and we therefore often find these monstrosities in gardens where, the more deformed and unnatural they are, the more highly they are considered. To this class belongs tulips with the large blossoms and jagged petals, here too, the variously formed roots and stems, the curled and variegated leaves which are no other than monstrosities brought about by art or chance.

However, we must distinguish between these growths and doubling (or double flowers), which multiplies some parts of the flower to the exclusion of others, thus rendering it sterile. Monstrosities, indeed, often bear seed as they neither increase nor decrease the parts of the plant, but merely give the ordinary parts an extraordinary and strange form.

The cause of the disease lies either in a too great abundance or a misapplication of the nutritive sap, by which the parts are enlarged or changed. The method of cultivation, replanting and warmth, are the usual sources of the disease.

A change in the methods can bring back the original form.

1st Species: *Monstrosity due to abundance of sap* is the most common form of the disease. The plants grow in too rich soil, and are given too great care.

2nd Species: *Monstrosity due to heat.* Change of region, variation of heat and cold exert great influence on the various changes in plants. Large fruits result herefrom, woolly (hairy), and perhaps too, the variegated leaves. Climate seems to have a special effect on size, taste, hairiness and color.

3rd Species: *Monstrosity due to external injury.* External injury is generally responsible for the bent, crooked, deformed and connate plants. The greater the injury the more deformed the plant as a rule becomes, provided of course it does not actually die.

4th Species: *Monstrosity due to a wrong pollination.* Crossing various plants under pollination usually results in forms totally different from all the others. However, as the majority of these are non-productive, they are unable to reproduce themselves.

Genus IV. Deformity. *Mutilatio*

Deformity means either that the plants are totally lacking one of their natural parts or else that it is small and imperfectly developed. It dif-

fers from doubling in that although in that disease, parts may be wanting, they are replaced or superseded by others; in the case of the deformed flowers some one part is entirely lacking without the others being enlarged or multiplied. This disease is especially prevalent on plants which have been transplanted from one region to another; however there are some which lack certain parts even when growing in their own soil.

It is generally believed that insufficient heat is the cause of the disease, as it is most often observed on plants foreign to cold climates; yet I doubt whether insufficient heat is the only cause. Our common *Glaux,* which each year beautifies our beaches with its small flesh-tinted flowers, grows, according to Adanson's testimony, in Paris, without petals. This cannot be due to lack of heat.

1st Species: *Deformity of the internal perianth.* This form is common with us on various plants from warm climates; sometimes no corolla whatever develops, sometimes it is so small that it does not deserve the name.

2nd Species: *Deformity of the stamens.* It is very unusual to find plants entirely without stamens, except in the case of the double flowers; however we often find fewer than is natural. On those plants which have 10 stamens (Decandria) this form of the disease is very common.

3rd Species: *Deformity of the stem.* We often find plants whose stem is lacking though that or-

gan should by rights be found. However in spite of this flaw they blossom and set fruit. Certain thistles are especially subject to this.

4th Species: *Deformity of the leaves.* The leaves are changed to mere scales which conceal the stem.

CLASS IV

Extraneous

Genus I. Necrosis. *Sphacelus*

Necrosis consists of the sudden stoppage of the growth; the plants immediately wither, become black, dry, and finally die. There is a general form of this disease by which the entire plant suffers, and a form in which some part of the plant is attacked and falls off without the remaining fresh parts suffering in any way, or being checked in growth. The disease is very common on *Ulex Europaea,* a withered branch almost always disfigures this otherwise handsome bush, making it little adapted to hedges.

The cause of the disease is a stoppage of the sap; this in turn is always the result of external injury. As soon as the sap ceases to flow, the plant itself, or the part affected must wither up and die; the blackish, dry color it assumes is the outer sign.

1st Species: *Necrosis due to cold.* In our northern lands it is very common for our plants to

freeze and die from cold; this is especially the case in the spring when the sap has begun to flow. The cold of winter very rarely injures our plants, but the severe frosts of nights in the spring expand and split the vessels already distended by nutritive sap, and the part thus attacked dies. The more beautiful, warm and moist the weather has been, the more harm is done during a frosty night. Plants growing in low, damp places always run more danger of freezing than those growing on high, dry places. On that account it is difficult to determine which plants can endure our winters; it is not always the very low temperature which kills the foreign plants, but as a rule the circumstances attendant on the cold, from which indeed, many of our native plants die.

2nd Species: *Necrosis due to heat.* Adanson (2) cites examples of strong trees which were killed in a single morning by the reflection of the sun's rays on the windows of a conservatory.

3rd Species: *Necrosis due to suffocation.* This form of disease is observed on pine and spruce trees whose lower branches cease to grow as soon as they are overshadowed by the upper.

GENUS II. Leprosy. *Lepra. La Mousse*
Adans., I., 45 (2)

Leprosy consists in the covering of the outer bark (of trees) with various species of moss; these

continue to grow there and impede the evaporation of the tree. These mosses apparently do no actual harm to the tree on their own account, nor do they deprive it of sap, as they rest on the outer surface of the bark alone without penetrating into the actual substance of the tree. However, when they appear in any quantity, they are always an indication of a poor condition of the tree. On young trees in rapid growth and on healthy trees, moss is rarely seen, but more often on the old and weak. There are many varieties of these mosses and they seem intended by nature to absorb sour stagnant juices, which form the greater part of their nourishment. They either have no roots at all or else roots too fine to penetrate into the soil or trees but remain solely on the surface. Under every period of heat or drought they dry up, show no sign of growth or life, until a subsequent moist period gives them new nourishment and new life. Everywhere where juices are checked and become acid and pungent to a degree destructive of all other plants, these mosses are found in great abundance, occupying every nook and corner. That is why they soon appear on flower-pots which are heavily watered and which have no drain, that is why we find them on acid meadows where only a few plants can thrive. However, in my opinion, these mosses do not actually injure either the meadows or the trees; on the other hand though,

they always indicate the presence of an unhealthy condition, and the poorer this is, the more gaily grow the mosses. This is beautifully illustrated on trees, for the mosses continue to grow even though the tree is felled or dead. Is it not possible that a rotten or acid evaporation from the tree also contributes to the growth of mosses?

It is customary to clean trees in gardens by using a scraper. The advantage thus gained is dubious unless other measures for stimulating the growth of the tree are taken at the same time.

Genus III. Lousiness. *Phtiriasis*

Lousiness is the accumulation of a countless number of plant-lice which attack plants or parts of plants, suck on them and destroy them. Every species of plant has its own species of plant-lice; these often appear in such quantities and suck so much sap that they contribute in no small degree to the withering of the plant. They are prevalent in especially great numbers on all plants whose growth is unsatisfactory, due either to unsuitable soil or to a disease of whatsoever nature. This is why we often find them on carnations planted in too rich soil, or on hops suffering from honey-dew.

There are many insects which we group under plant-lice, those for instance which Linné, the Knight, designates as *Aphis, Coccos* and *Chermes;* all of these injure our plants in the same way, by

sucking the sap. However, *Aphides,* which are the most numerous, are the most injurious, and it is they which are generally known as plant-lice. They attack all parts of the plants, even the roots below the earth, still they prefer the young shoots or branches from the preceding year, where the bark is fresh and tender. Other forms cause the leaves to assume lumpy, warty excrescences, as seen on currant and other bushes, and still other forms attack the fruit growing on the plant.

1st Species: *Plant-lice on healthy plants.* It is very uncommon to find a plant totally free from plant-lice, but as long as the plant is in good development and there is sap in sufficient quantities, they only do a minimal amount of harm; compensation is soon given in the constant flow of the sap.

2nd Species: *Plant-lice on delicate plants.* As soon as plants suffer, nature does all in her power to dispose of them so that the stage she sets may ever be fresh and perfect. Plant-lice are of the greatest assistance in this, since they take possession of, suck upon and totally destroy weak plants, or those growing in unsuitable soil.

Almost countless are the remedies for plant-lice mentioned in agricultural publications; however these cannot be used, partly because they crave such great expense and trouble and partly because they are of no avail. If our plants are in a suffering and weakened condition, it is almost impos-

sible to get the better of plant-lice which are almost myriad in their fertility. It is then better to prevent the disease by a wise choice of methods of cultivation, than to use remedies against it when it has once gained access.

Genus IV. Langour. *Deliquium*

Langour consists of the cessation of all growth and feeling in plants. The leaves hang limp; they seem withered and dead, but in a few minutes they recover and are as sound and healthy as before. I do not know whether I dare to group *langour* among the diseases of plants as it does no harm and seems rather to be a quality than a disease. Moreover it is seldom observed, we know but few plants attacked by langour, some *Mimosa* species for instance, and *Oxalis sensitiva,* and perhaps a few others.

The cause of the phenomenon seems to lie in the sensitiveness of the internal pithy parts. Adanson (2) claims that it is due to strong contraction, for the parts of the plants cannot be replaced in their former position without breaking them. As yet we know too little about the nervous system of animals and plants to be able to show all the effects it may have.

Genus V. Wounds. *Vulnus*

Wound is the name given to every injury of the external parts, whatever the cause may be. There

are, then, many kinds, and our plants often suffer much under them. I will not include here the various cuts which we administer, partly to their benefit, partly to their improvement,—the pruning of trees for instance and others which form a very necessary part of our horticultural science.

1st Species: *Wounds caused by animals.* Creatures, both wild and domestic can cause considerable damage to our plants, especially in our forests by breaking off and tearing down the plants. Sometimes they gnaw the bark and sometimes crop the tips of the young bushes thus rendering the tree a permanent cripple. It no longer shoots up high into the air but spreads; the side branches grow out and the trunk is forever bent and crooked.

2nd Species: *Wounds due to insects.* Insects do the greatest harm both from the point of view of economy and of beauty. They injure and destroy everything. Some years insects alone are sufficient to destroy all hope for a rich harvest. There is no part of the plant which they do not attack. Some devour the roots, some attack the bark, some destroy the leaves while others injure the blossoms and the fruit. If we had a closer acquaintance with the nature and characteristics of these small pests, we could more easily prevent the harm which they do.

3rd Species: *Wounds due to parasitic plants.* Parasitic plants are those which plunge their roots deep into the substance of another plant and thus drain it of some of its nutritive sap. There are various kinds of parasites and they attack different parts of the plant. Some fasten to the roots, others hang to the branches and trunk; among these *Cuscuta* and *Viscum album* are the most common and the most pernicious here in Europe.

NOTES BY ERNST GRAM

THE system as given in the introductory paragraphs is slightly altered in the text, Fissures being added to Class V which is there called Injury. Furthermore the sequence of Classes II and IV is changed.

Class I, Genus II. Fabricius's opinion of smut, that it is neither caused by any kind of fermentation in unripe seed grain, nor by the soil, but by "something organized," *i.e.*, something living, an organism, forms one of the good reasons why he is ranked among the fathers of Phytopathology. He admits that nothing is definitely known, but in his hypotheses he takes a step farther than Tillet and his main source, Adanson (2). The latter author examined the spores under the microscope, and concludes that smut is caused by a plant similar to "Vesse-de-loup" (Lycoperdon), cold and moisture being the primary, the spores the secondary cause. A contemporary author, Aymen, even assures that he has produced smut with spores from Lycoperdon, and in 1775 Bjerkander calls smut *Lycoperdon tritici.* The indications of worms in smutted grain may possibly be due to

confounding smut balls with the galls caused by *Tylenchus tritici.*

Class II, Genus I, Art. 4. Acaro telario: Red spider (*Tetranychus althaeae*). The grouping under Choking apparently suggests, that not the sucking, but the web of the mites is the cause of the wasting.

Class III, the "Discharging" referred to in the introduction. Genus III, Honey-dew. When Linné (11, pp. 49–50) indicates the larvae of the lepidopter *Phalaena humili* (*Hepialus humuli*) as the cause of honey-dew, it is based on his own observations and experiments. Linné actually reared the imagoes, which deposited plenty of "seed," strewed this about his hop vines, after which he found next year "—that their larvae had attacked the root vigorously. I found moreover a great shoal of lice or *Aphides* on the leaves. Thus I think that the said lepidopters devour the root, making the hop vine sick, after which the lice or *Aphides* appear." The description of Fabricius only covers the honey-dew, the wording of Adanson covers essentially the mildew or perhaps also sooty mould. The term used by both, le Givre, properly means hoar frost. As late as 1796 Rafn (13) describes mildew as "spots first sticky later as a meal." When these spots are sticky the disease has been called honey-dew, and ascribed either to insects, or to dew, etc. "I estimate it in

no wise to differ from mildew, except in its relation to the duration of the disease as the sticky liquid is changed by the heat into a dry or mealy substance."

This confusion of mildew and honey-dew, perhaps due to the frequent coincidence on hops, is but slowly disentangled. For several species of downy mildews Linné applies the name *Mucor erysiphe,* thus undoubtedly classing them among the fungi. Hedwig used Erysiphe as an unpublished genus, which was adopted by Lamarck and Candolle (8), but the mildew of hops was not classified as *Erysiphe macularis* before 1829–32, by Elias Fries.

But from antiquity honey-dew has been considered a perspiration, either of the firmament (Pliny) or of the earth (Galen), and as an exudate of plants it goes bravely on. A few years before the attempt of Fabricius, Lerche (9) personally discovered the true, modest nature of honey-dew, but although his thesis was well known it was buried by many kinds of strange theories. Of these, one proposed by Goethe, that plant-lice subsist on honey-dew and multiply the livelier, the more honey-dew they are offered, may be called the most preposterous. In the 40's, Kaltenbach confirmed the observations of Lerche, but still a generation later such prominent scientists as Hooker and Darwin—and among the Phytopa-

thologists *Sorauer*—considered honey-dew to be a plant secretion. The proper interpretation actually seems to have gained credence from a work of Büsgen (3) 1891. For sake of completeness it may be remarked, that honey-dew has also been applied to the conidial stage of ergot (Claviceps).

Class IV, Genus II, Art. 4. Putrefaction in saffron, *mort du safran,* caused by *Rhizoctonia crocorum* was described, 1728, by Duhamel (5).

Genus III, Canker. Descriptions of Adanson and Fabricius do not cover the usual symptoms of *Nectria galligena* canker.

Genus IV. Rust. Adanson's short paragraph on rust is verbatim: "La Rouille (Rubigo of Teofr.) est une poussière jaune de Rouille ou d'Ocre, répandue sous les feuilles, sur-tout de Rosier and du Titimale à feuilles de Ciprès (*i.e.,* Euphorbia cyparissias). Elle reconoit la meme cause que le Jivre (*i.e.,* mildew and honey-dew), et pouroit etre écartée par les memes moiens." Duhamel (6) seems most inclined to consider rust as a juice, exuded through the rust pustules and then dried (Vol. I, Book III, Chapter II, Art. I).

Class V, Genus II, Galls. The opinions of Fabricius on ergot cannot originate from the book of Adanson cited, since this author mentions its occurrence on rye and other Gramineae, and on "souchet" (Cyperus spp.), as the great venomousness, and speaks of its prevalence in moist

seasons. Ergot poisoning had been frequent in Holstein in the century previous to the appearance of Fabricius's paper, but while ergot had earlier been indicted, in this period many physicians considered it harmless (10). Neither can the information given by Duhamel (6) be Fabricius' source. What the author meant by ergot in Lotus and Cerastium is not known; possibly a gall (*Dasyneura loti* DG) and the anther smut (*Ustilago violacea* Poetsch.), respectively, may have given rise to the statement.

Genus IV, Glaux, Adanson (2), page 112.

Class VI, Genus IV, Mimosa, Adanson (2), pages 56–58.

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FELICE FONTANA
SOMMO NATURALISTA
POMAROLO 1730-1805

Phytopathological Classics

NUMBER 2

OBSERVATIONS ON THE RUST OF GRAIN

BY

FELICE FONTANA

1767

A Translation by

PASCAL POMPEY PIRONE

Fontana, F. Osservazioni sopra la ruggine del grano. In Lucca 1767. Nella stamperia Jacobo Giusti. Con Lic. dé Sup.

With a biographical sketch and translator's preface by

PASCAL POMPEY PIRONE
Cornell University, Ithaca, N. Y.

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THE AMERICAN PHYTOPATHOLOGICAL SOCIETY thanks the translator, Pascal Pompey Pirone, Graduate Student in the Department of Plant Pathology, Cornell University, for his excellent rendering in English of this classical work of the famous Italian scientist, Felice Fontana.

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FELICE FONTANA

Felice Fontana, famous Italian physicist and naturalist of the late 18th century, was born in Pomarolo, northern Italy, on April 15, 1730. He began his studies at Rovereto and continued them at Verona, Parma, and at the Universities of Padua and Bologna. He then went to Rome and Florence.

Fontana was chosen Professor of Philosophy at the University of Pisa by Francis I, Grand Duke of Tuscany. He held this chair until Archduke Leopold called him to Florence to build the museum of physics and natural history, which, even today, is one of the principal ornaments of that city. He invented many physical and astronomical instruments and also prepared with colored wax a form of the human body with all its component parts.

All of his time, however, was not spent in mechanics. He published many dissertations on physics, chemistry, and physiology. He wrote various articles in both Italian and French on blood globules, on the circulation of sap in plants, on the "tremelle" of Adanson, a species of zoophyta, on snake poisons, and also a few articles on plant diseases.

Very little is known of his private life aside from the fact that he frequently attended scientific-society meetings. He was loved by all who came in contact with him. Fontana died on March 9, 1805, after a great deal of suffering from injuries received in a fall.

Du Bois-Reymond once said, "Felice Fontana spread the light of science to all of Europe."

P. P. Pirone.

TRANSLATOR'S PREFACE

The following translation of Fontana's book was undertaken as a part of the course in the History of Phytopathology given by Professor H. H. Whetzel to graduate students at Cornell University. A copy of the original book was obtained from the Library of the United States Department of Agriculture at Washington, D. C.

Fontana's observations are very remarkable when one considers the period in which they were made. Were it not for the date placed at the front of the book, which removes all doubt, one might be rather sceptical that he was able to discover and so exactly trace the fungous nature of the rust many years before Persoon and DeCandolle mentioned it in their works. Although Fontana did not understand the relationship existing between the two forms of rust he described, there is little doubt, after one examines his fine plate at the end of the book, that he worked with *Puccinia graminis* Pers.

We have presented here what is believed to be one of the earliest contributions to the true causal nature of the stem rust of wheat. These observations were made in the course of a year.

Fontana's name is not to be forgotten in the science of plant pathology, for he stands forth as one of those geniuses whose misfortune it was to have lived long before the world was able to understand them.

P. P. Pirone.

To the Most Illustrious Baron Van-Swieten, Commander of the Order of Santo Stefano, Court Medical Councilor of Their Imperial Majesties and Perpetual President of the Vienna Medical Faculty, etc.

It seems fitting that a small book dealing with new observations concerning a little-understood disease of a plant, to the preservation of which ancients consecrated a Goddess, should be dedicated to no one other than that incomparable man who has enriched Medical Science by new, important discoveries and observations of the nicest type and recently through his profound practical and theoretical knowledge, has made the most fortunate application of it to the cure and conservation of a precious Life,[1] on which depend the destinies of Europe and which is of great interest to Humanity. You understand then, most worthy Sir, that this little book of mine could not be dedicated to anyone but you. I hope you will not disapprove of the respectful liberty with which I discuss in this work the opinions hazarded by many eminent writers on this subject, since, in more than one instance in your distinguished works, summoning for examination the hypotheses of the most celebrated masters of the art of healing, you confess the necessity of warning the reader, with proper liberty and wise council, against the influence of the celebrity. Therefore, the not unimportant discoveries, which I appear to have made on the most fearful and harmful disease of grain, should merit your approbation. I should be more than certain, then, of having merited the approval of all great men whose judgment cannot disagree with yours, just as their worth cannot disagree with yours.

FELICE FONTANA.

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[1] Translator's note: Van-Swieten was made a Baron for saving the life of the Empress of Austria, Maria Theresa.

To him who may wish to read.

On the 10th of June of last year, I discovered that the rust, which had devastated the lands of Tuscany, is a grove of plant parasites that nourish themselves at the expense of the grain; and at that time I pointed this out to many of my friends here in Florence, and a few days later I wrote about it to the most learned men in Italy and other parts of the world. About this time a celebrated professor at Pisa, to whom I had mentioned it in a letter, honored me by writing a short article on it in the Venetian Gazette of Natural History. Be this said in all truth lest some believe that I may have profited from the pamphlet which appeared in Florence three months later. Therefore, I present my observations in this form to the public now. I present them to you, courteous reader, assembled in a small book because I know it is of little importance to you to know all that has been written or imagined on this question up to this time, and which I could have placed together and collected in a large book at little expense and work. Since it is beginning to be recognized that one may know the history of the thought of all men without knowing how to think, it appears that even the secondary and common class of initiates in Philosophy is being cured of the mania of quotations from both Greek and Latin texts, especially when they treat of works registering the facts of nature rather than the concepts of men. I certainly could have filled up a hundred pages alone with the citations of ancient authors, naturalists, philosophers, and poets on this subject and another 400 pages on the thought and suggestions of modern workers, and by covering it over with a mass of Greek and Latin and if need be with Punic and Palmyran, I could produce without effort and by mere copying an enormous and deadly volume.

But I willingly renounce all this glory and shall leave to others the courage of imposing so severely on your sufferance. I know that there are those who wish to see even a physical truth expressed in verse; and I once heard a famous dialectician seriously affirm that all the queries of natural history can be answered with the verses from the Georgics of Virgil. And are there not those who find the answers to all questions in Homer, even in our day? But enough of this.

I respect too much the Century of Philosophy and of Reason to wish to do it the injustice of believing it prejudiced in favor of the pedants.

"It is by well reasoned and prolonged experiments that one forces nature to disclose her secrets; no other methods have ever been successful, and true scientists must regard older systems as ancient dreams and must read the majority of modern treatises as one reads romances. The collections of experiments and observations are in truth the only books that can add to our knowledge.

"Let us always, therefore, amass experiments and turn away, if it be possible, from any idea of system, at least until we have some knowledge. We shall surely find some day a place for these materials, and, even if we were not fortunate enough to build a complete edifice, they shall certainly help us to build a foundation and perhaps to advance it, even beyond our hopes."

M. Buffon Préf. de la Stat. des Végét. de M. Hales.

PERSUADED by reason and taught by experience that the study of the errors of men is an idle and sterile occupation, if not a real loss of time for the Philosopher who seeks to discover the truth, and that hypotheses may for a time stir up the imagination of a poet but can not satisfy the intellect of a thinking man, therefore, since I wish to discuss the rust of grain and relate to the public what I have examined, seen, and observed on this point of natural history concerning which many and various things have been written to the great scandal of the sceptics, I have determined to point out only the principal hypotheses that are to be found on this subject in the works of naturalists, at the same time indicating and discovering their incoherence and falsity and then immediately setting forth that which I have decided and established, that which appears to me correct after having conducted many observations and long severe examinations. These observations were made by me last year, when a sudden severe attack of rust ruined the grain all over Italy and especially in the province of Tuscany.

From the beginning and even now many philosophers believe that the dust, which composes the rust, is a mass of very tiny animal eggs that are deposited on the leaves and stalks of the grain plant by a certain species of small worms or insects. It is also claimed that there were seen (and what things are not seen by the prejudiced mind?) emerging from those eggs, large families of living creatures which are fatal to vegetables and deadly to grain. Such an hypothesis could truly appear to be confirmed by some observations made by the industrious Ginanni, who assures us that he has often found very small bugs in the granulose powder of the rust. But such an opinion cannot convince an observer who has an unprejudiced mind, since the worms mentioned are found very rarely and are observed (when they are found) subsequent to the dews. They are found to be innocent and absolved of any harmful influence concerning the rust and are present there because of the shelter afforded them. The following observations, which I repeated many times, always with the same result, will break down an already weakened hypothesis.

The granules of rust were pressed tightly between two plates of glass so that the supposed eggs were broken; with a very powerful microscope I observed them being crushed one after the other. I can certify that no liquid or gelatinous humor came out of any of these, as no doubt would be the case were they animal eggs; that great force was necessary to crush them and that they were homogeneous throughout, being filled with a solid and resistant substance. The structure, then, of the powder that forms the blackish rust spots, as we shall see, resembles anything but an animal egg and is attached in such manner to the stalk and leaves of the grain plant that it does not agree with any proper or scientific character of ovules left there and deposited by whatever species of insect.

Other philosophers have advanced the theory that these minute bodies of the rust powder are, on the contrary, probably real live animals, being deceived perhaps by the tailed shape shown by the granules when observed under the microscope. A certain movement observed when they are examined in water contributed further to deceive some of the observers I have known. In the natural state the small granules composing the rust powder are sticky and slightly viscous and therefore remain united and piled together. When these are placed in water, the movement of water from grain to grain divides and separates them and induces such a motion among them that one who examines them hastily or who is not accustomed to seeing the spontaneous motion in the Infusoria can easily be deceived. A movement of such a nature between the granules is purely mechanical and is the ordinary effect of the water. This movement eventually slows down and is stopped, instead of increasing as in the case of aquatic animals; it is never observed in the rust that is still fresh and wet on the plant.

There are some who believe that the rusty powder of the grain is the work of very small microscopic animals, which alight on the plant during certain favorable atmospheric conditions. But this hypothesis also is contrary to experience, since these insects are rarely found or when they are found, as happens sometimes after a dew, no rust is present on the plants. Moreover, the complicated structure and great regu-

larity of the black rust make such an opinion improbable, and, above all, the attachments of those granular powders, show them to be anything but the work of insects.

Another theory concerning rust must be examined, which, sustained and reinforced by the authority of the greatest Italian philosopher, a man of great discoveries, and few mistakes, led many thinkers into error. The theory is that many diseases of grain, such as blight, smut, and rust are caused by solar rays, which, united and brought to a sharp focus by the drops of water, on the tender small plants, burn, scorch, and consume the grain and the leaves. He believed that the droplets of humor exuded from the plant or deposited from the atmosphere, act as burning lenses, which unite the parallel rays of the sun and condense them into a small point on, or a little below, the epidermis of the plants, where they scorch and blacken the integuments. This ingenious hypothesis also is subject to the fate of all the others, namely, that it is not in accord with the facts. There are certain facts that show that neither mildew nor smut originates from those drops of water that are transformed into burning glasses; and, in our case, it is certain that rust appears only when the sudden cold of the night is not properly proportioned to the heat of the day and when dry heavy fogs appear, at which time the dreaded drops do not exist. Another current idea is that the diseases of grain appear after the dews and rains, but precisely the opposite occurs; moreover, all the other species of plants should be visibly scorched or burned since the above-mentioned drops are found on all plants. It is certain and beyond doubt that the very minute drops of humor could never collect such an abundance of rays as to blacken and char the most delicate and fragile part of the tenderest plant. It is absolutely necessary that the lens have a considerable diameter and that it collect a sufficient quantity of rays which when united in a common focus have the strength and activity to burn. This diameter of the lens must be incomparably greater than that of the very minute drops covering the plants. Out of my own curiosity, I have many times sprayed water on the most tender living plants and have done the same on the leaves and stalks of already dried and completely withered plants; I have then exposed them for a very long time to the

very hottest rays of the sun at midday and have never noticed any burning or blackening in any part. I have many times placed tiny lenses and beads of fused glass on various leaves and I can assure you with certainty and without fear of error that the light, collected and condensed in their foci, was entirely harmless and inactive.

Finally, the most common and accepted theory, even among modern observers, is that the powder of the rust is only a humor or an exudation of the plant that arrests itself and degenerates into granular powder during certain periods and under certain circumstances; or, as others believe, it is a large and viscous humor left there by the air at certain times; or rather, as the celebrated Tillet claims, it is a sour and deadly humor dispersed and radiated by the air during cloudy periods, which corrodes the tender fibers of the leaf and stalk, causing a diffusion of the fatty and oily juices, which, upon drying, are converted into a reddish orange powder; or, finally, as Hales claims, it is the nutritive juice of the filled plant that becomes stagnant and sours in the vessels, which, in turn, infects and spots the plant. A single observation made on the structure of the powder is sufficient to prove all these hypotheses false. If that powder were a humor left by or gathered on the grain plant, it would be formed of globules and would never appear to be made partly like an egg and partly with the shape of a real plant with a cup, stem, and acorn, as we shall see later on. An uncontrolled and irregular fermentation, a humor that drys and hardens, would never be capable of forming bodies so regular, and so constantly regular, as are those composing the rust. The attachments that are constantly observed on the black and red rust and the mutual supports of those small bodies could never result from those causes that have been imagined by the naturalists who did not consult nature. *"Quam bellum est velle confiteri potius nescire quod nescias, etc."* (What a fine thing it is to be willing to admit you do not know a thing that you actually do not know, etc.)

But already, I feel weary of being in the land of dreams and realize that I am impolite in detaining the reader longer

than is fitting in discussing them. Because of the disagreement and uncertainty among the most modern philosophers, I believed that the solution of the problem could be found only through correct observation and a more exact and diligent examination of the powder of the rust. I collected a good quantity of the said powder, which I took from a great many grain plants, sometimes by shaking and sometimes by removing with a penknife where the plants seemed most spotted and discolored. When placed on a piece of glass and examined with powerful lenses, I observed them to be piled in various groups; I was unsuccessful in separating them with a needle point, since they appeared to be covered by a thickened humor that kept them together. This powder was composed of two kinds of minute bodies; some were ovoid, and practically of the same form and size; others were very long and, in some respects, similar to minute fungi or to ordinary nails.

The bodies of the first kind, that is, the ovules, generally appeared opaque or very slightly transparent, even in the strongest and most active rays of the sun. They were ovoid for the most part; some resembled ellipses, and a few others were slightly pointed and irregular at one extremity. Their color was a rather dark reddish yellow, with dark extremities. Many of the bodies were transparent at the two extremities or points; others had thin translucent rings around them with an opaque reddish yellow ovule in the center. This more highly colored oval spot is seen in the center of the eggs that have the transparent rings around them, having the appearance of an opaque egg at the center of an elliptical roll.[2] Some of these ovules are spotted with dark points above the colored part but never above the transparent part. I have found a few that were translucent, colorless, but these are so very rare that less than one in a million is found. When they are piled upon each other they become much darkened and almost black in appearance; when separate and divided they are yellowish red or the color of yellow and opaque glass.

[2] **Translator's note: Roll, i. e., cake or biscuit.**

The other kind of rust powder had the appearance of fungi in groups, or mounds of nails with large rounded heads. These tips or heads were attached to a large long cone, giving the appearance of acorns set in cups, which, in turn, were attached to very long stems. These nails were so bunched and tightly united together that a jarring of the glass slide on which they were placed could not separate them, and the point of a needle was capable of separating the bunches only enough to form smaller ones. Here and there, however, were seen a few that were totally isolated or in contact with only two or three others. The same was observed in the other kind of rust, which is composed of small eggs; some may always be seen that are isolated from the rest or attached to a few others, depending on the amount of moisture in the powder, the manner in which it is removed by the needle point or knife, and, finally, on the method of preparation for microscopic examination. The nails appeared darker, and blacker where they were massed, and transparent when there were few. When few are together or when they are observed singly, their color is yellowish red; perhaps somewhat darker than that of the little eggs, especially the head of the nail which appears black and opaque when it is united to several others and is observed in reflected light; the cup in this case is also very obscure and nearly black, but when it is seen isolated it has the usual yellow red color. The stem of these cups is always transparent and translucent, especially in the center and towards the tip, at which places it appears to be made like a crystal or ice.

If we divide these bodies of the nail-like rust into three parts, namely, the head or acorn, the cup or cone, and the stem or root, we see first that the heads are not so uniform as the eggs either in size or in form and shape. Some are round, being perfect spheres or little balls; others are very long and almost cone or pear-shaped, since they taper to an acute point; these made up the largest part of the types found; others are somewhat irregularly ovoid with apices that have a bulge or protuberance at times and that end in a blunt point; and others are long and olive-shaped, though very few of the latter are ever observed. I have found a few (but

these are very rare and are found only by chance after a thousand fruitless attempts) with a peculiar form and structure, which have a long thin horn or spine that ends in a very acute point, projecting at the center of and lateral to the head. The heads or acorns appear to be set into a cup or cavity which hides a part of them from the eye of the observer.

The cup is a long conical body, more frequently straight than curved, and at times somewhat irregular; in some nails it is bulged, while, in others it is thin and slender. A darker ring or small circle appears at the uppermost part of base of the cup, inside of which there appears to be a cavity into which the head or acorn partly sits, as has been mentioned. Sometimes the ring is larger or wider than the acorn that it holds, but more often it is much smaller. In some, the ring appears to stretch outside of the acorn; in others it is very small and appears as a thin slender collar that supports a large head. Some cups are very long and widen out towards the tip because of a large bulge found towards the middle; also others are very short and decrease in size from the base to the tip. Once I chanced to observe a cup without its acorn, which happened to be in close proximity to a little egg, and I could see very well that they did not belong to the same plant. It is, moreover, true that it is extremely rare that one can make such an observation but in such a case one can observe a rather shallow cavity the form of which corresponds to the base of the acorn that had rested in it. All the stems usually end in a somewhat obtuse or blunt point. It is true that a large number are found broken off near the tip, probably when they are detached from the grain plant. At times the stems are conical, very slender, widening towards the tip and transparent throughout their length, which equals the length of the cups, which in turn are usually longer than the acorns.

When a droplet of water is placed on a mass of those fasciated nails, they begin to move in various directions, and they separate into smaller bundles as the water creeps in between them and neutralizes the effect of their natural glutens. These are all united together as much as possible, since very

few are observed alone; and, even when one tries to separate them with a needle point, very few totally isolated bodies are found. The movement lasts for a few minutes, until the water has fully permeated them; the motion is derived entirely from the water that permeates them and, as I observed a thousand times, it is certain that the bodies that have already been separated are motionless; nor does it appear that they visibly imbibe water and increase in size, even though kept in water for a long time. During the time that such a movement lasts, the tailed bodies, which resemble the spermatozoa in some respects, can easily be mistaken for minute living eels; and, in fact, I have known more than one person, whom I have put to test, who believed such a thing. But such motion is certainly mechanical, and is produced by water, since no spontaneous movement or tendencies or directions can be found as may be observed in the case of all microscopic eel-like animals; and the motion does not continue but ceases after a short while, even though they are still in the water. The same may be said of the eggs, which also move when they are placed in water, although their movement is less active and of shorter duration because of their shape and minuteness.

The observations made up to this point lead one immediately to suspect that the nails are very minute plants that nourish themselves at the expense of the grain. But, after all, this is only a suspicion and an hypothesis. It is true that some light could be shed by chemical analyses of the rust powder, by determining whether an acid or alkaline humor is given off, and whether it is vitrified or calcined by fire. In fact, Ginanni has subjected the dust to fire and assures us he had extracted a strongly acid fluid. But these experiments are always somewhat dubious and apt to lead into erroneous conclusions anyone who cannot modestly suspend his own judgment and make up his mind to appear as second in importance. For example, it is known that vegetable substances like the galls, form ink when they are united with vitriol. In order to determine whether certain vegetable excrescences that are found on trees were vegetable or animal matter, Count Marsily united them with vitriol and succeeded in obtaining ink; he hastily and without proper circumspection con-

cluded that those tumors were real galls of vegetable origin. Lemery succeeded in making ink from animal excrement and finally from human excrement, showing that vegetable matter retains its ink-making properties after it is passed through an animal body where it is broken down and digested. The great French observer, M. Réaumur, succeeded in converting Marsily's galls into real insect galls.

A vegetable substance can in a thousand cases retain either many or few of its original properties, even though it seems to have changed into an opposite substance and to have acquired new and different properties. Some workers have been able to extract a slightly sour liquor from putrefied meat, and the celebrated chemist, Vallerio, has even succeeded in vitrifying (crystallizing?) animal substances, as, for example, the solid part of ox blood. These experiments contradict general conclusions which are hastily deduced and elevated to the dignity of axioms.

But, even though those experiments prove that the rust is a vegetable substance, there would still be much obscurity and uncertainty, since we would not know whether those plants are foreign to grain, whether they are a humor of the stalk or the leaves which is collected or fixed, or whether they are a dewy substance or some other humor that is deposited by the air under certain conditions, and then solidifies and enlarges by evaporating the most delicate and volatile parts.

I had all these ideas fixed in mind and the figure of the small plants found in the rust was always impressed on my thought; I suspected that they really were parasitic plants that were carried by the wind and lodged in a suitable and adapted place to feed at the expense of the grain. It is true that those bodies terminated in a simple, unbranched root, unlike other plants, which are branched; however, at the present time we know that there are plants that do not have roots, as in the case of the truffles, the "*Care-folium of Paracelcus,*" i. e., the "*Flos coeli,*" or the *Nostoc,* which dries up as soon as water is deficient and is barely visible, even though a little water be sufficient to enlarge it and renew its life and vigor.

We also know that there are other plants that terminate in a simple point and are unbranched. *Lenticula* and *Lenticularia* of Micheli do not have branched roots but end in a slender thread that appears to enlarge as one proceeds from the leaf and whose point enters into a larger conical sheath. It is true that these are aquatic plants; but I do not see why the rust cannot be considered in the same light, as a plant that lives on the juice of grain surrounding it, just as *Lenticularia* lives in the surrounding water. Micheli, in his work Nova Genera Plantarum, page 128, describes, among the plants, *Suillus,* a very large mushroom, which the Italians call the Malignant Porcine Bodkin, whose roots terminate in a point. Several other smaller ones are figured in table 80 by Micheli; on page 204 he describes the fungoid types, some of which resemble in some respects, the long bodies of the rust. Some very small plants resembling the rust may be found among the *Clatroidi* in table 94. On page 84 the same author describes some small lichens that have, in addition to the cup, also at the tip a head or cap, in which respect they closely resemble the rust. Although these plants have something in common with the fungi, lichens, etc., they are very different in the remaining parts; and, though we do not have sufficient evidence to suppose them real plants, we have even less evidence to suppose them fungi or lichens. They might possibly be related to the molds described by Micheli, but those plants are far more tender than the rust and do not have the uniform structure similar to that of the rust. Then, too, we find seeds or buds in all the plants such as the lichens, fungi, and molds; I am still uncertain whether seeds are present in the black rust; however, it has not yet been shown whether the "Byssi"[3] have real, at least visible seed, and, besides, it is very certain that they are not found in many other plants.

In the final analysis, even though all these facts further the opinion that the rust is a mass of parasitic plants, they do not positively prove this point. It was necessary to examine more closely, combine, compare, analyze and compel Nature to reveal her secret and catch her in the act. In the course

[3] Translator's note: Probably white mycelial masses.

of my experiments I have observed many times a singular and little-understood phenomenon. I observed with the microscope that the dust, which I gathered from the stalk or leaf with a knife, was made up at times only of small eggs and at other times of piled nails without a single egg attached to them. Such an occurrence made me suspect that there probably were two kinds of rust or at least two different forms of the same. With this suspicion in mind I examined the rust on the stalk more closely and discovered two kinds of spots, which were very different in appearance, some having a very black color, while others were yellowish red or cinnamon. I then examined the dust from the black-rust spots and found to my surprise that it consisted only of nails or plants, among which I could not find a single egg. I repeated this observation, always with the same result, at least a thousand times; that powder always consisted of the bundled and heaped plants and never of eggs. On the other hand, I always found that the powder obtained from the reddish spots contained only eggs and never the ordinary plants of the black rust shown in figures 1 and 4.

The grain plants that I examined, then, were attacked by two kinds of rust; one that appeared perfectly black to the eye, and the other, the color of rusty iron or a reddish yellow. The former was composed of nails or long bodies with stems, while the latter consisted of eggs. They both were practically the same reddish yellow when examined with a powerful lens; and, if there was a little difference, it was found in the heads of the nails which appeared darker, that is, more laden with the reddish yellow color; but they never appeared black, like the powder from which they were taken.

After the fact had been established that two species of rust attacked the grain, and that the black rust contained bodies with long stems, resembling small worms, one observation remained to be made, which alone could determine with the most excellent physical evidence whether those bodies really were plants. It was necessary to determine whether they were fixed in the stalk or leaves, as plants are in the earth, i. e., to determine whether they were growing perpendicularly with their stems attached inside and the heads or caps outside, as

is the case with lichens, fungi, and even with molds. This observation, combined with all the preceding ones, would leave no doubt and would compel the less measured and less temperate sceptics either to believe them real, living plants or to exclude impolitely from this rank the lichens, fungi, and molds, thus doing the botanists the wrong of degrading from the order of living organic bodies those which they themselves have elevated to it with legitimate and non-usurped authority. After cutting with a knife several times into that part of the stalk which was spotted with black rust, I observed with a microscope the cut fibers and the dust adhering to them. One day I observed ten of the described bodies attached to a round small fiber. They were attached by their stems to the fiber or strip and their heads projected out, as is shown in figure 2.

Although this observation was of much weight in determining the nature of the above-mentioned bodies, it was not final because the fibers to which those bodies were attached by the stems were very rarely seen and it was difficult to determine exactly when they were observed on the glass, whether those plants were growing perpendicularly on the stalks. To solve this problem I thought of examining the spots of rust on the stalk itself without laying them on the glass. First of all, I wanted to compare the spots as they appeared to the naked eye and under the microscope. The black and the yellowish red spots as well, were spread in long streaks parallel to the leaf and stalk fibers, and were never placed in a transverse or opposite direction to the fibers or vessels of the plant. These longitudinal spots, which, because of their minuteness, sometimes appeared as dots, were rough to the touch and seemed somewhat raised on the stalk or leaf, even without the aid of the microscope. The stalk and leaves appeared to be penetrated with large or small furrows, depending on the size of the spot when the powder especially of the black rust, was removed either by shaking or beating the plant or especially by soaking it in water for some time. The powder was viscous and sticky and stained when rubbed between the fingers or linen cloth, where it easily stuck.

The epidermis was broken where the small cavities or fur-

rows of the leaves and stem contained the black or reddish yellow powders, and it was raised above the spot in many places, giving a "torn-dress" appearance to the furrow and the powder it contains. This irregular epidermis does not cover the rust fissure entirely, but only in part and this around the circumference, while there is a broken and torn appearance in the center; the epidermis usually exposes the black rust more than it does the red.

After thus examining with the naked eye the two kinds of rust, as it appeared on the plant, I examined it with great care under the microscope and in the same position. When examined with two lenses of a Cuff microscope, the black-rust spots had a saddle-shaped appearance and seemed to consist of a black globular material, for the black spots were raised in the center, forming a ridge or curved hump. Where the spot was blackest and largest, it was either entirely free of epidermis, or, if there was any membrane at all, it was torn and small in extent and left the greatest part of the black spot uncovered. The epidermis covered the smaller and lighter spots, arranged in narrow stripes, in many cases covering them so well that the small granules of black rust could be seen only through some small longitudinal fissures that sometimes were extremely difficult to find, even after very diligent perseverance. I have never been able to find a single pustule of rust above the epidermis. I must not deny that at times, though very rarely, I was unsuccessful in observing an opening in the epidermis although on looking through it I perceived underneath the usual heaps of black powder that I took out with a lancet, in order to be more certain. In all instances where the epidermis was seen covering the powder, either completely or in part, it was always raised notably, forming a tumor or swelling, depending on the amount of powder under it. It is not difficult for an observer with a microscope to believe that the unruptured epidermis, which covers the black rust, had real plants under it, nor could he believe that the rust is a humor given off by the canals of the stalk or leaves of the grain plant merely because he does not understand the means of their entrance into the plant. This is the point on which the great mind of the Tus-

can observer, Francesco Redo, failed. He observed that animals lived under certain vegetable excrescences and since he could never find an opening through which they entered, he concluded that the plants had the power of producing live animals, but, today, everyone knows that the humor that diffuses from an injured plant can easily and perfectly close a wound.

I finally observed a great number of spheroid bodies that touched and were piled one upon another, after examining with very strong lenses, those masses or mounds of black globular powder on many stalks and leaves of the grain plant. On carefully turning over a piece of stalk on which they were attached in great quantity, I perceived that all the spheroidal balls had a long stem with the point of which they were strongly attached or fixed on the stalk.

With the aid of a powerful lens they looked like a crowded grove of trees planted at equal distance on the above-mentioned furrows. The heads were raised and, like the stems, were closely pressed against each other. They touched each other in such manner that the plants on the edges, farthest from the center of the crowded grove appeared slightly oblique and inclined, while those away from the edge and closer to the center were all erect on the grain stalk and planted perpendicularly, like other ordinary plants.

It is certain then that the black rust is made up of very small, regular and compacted parasitic plants. The celebrated Henry Backer discovered a microscopic plant in the sand of the Thames which very closely resembles our plant. This plant is larger, but, as can be seen from figure 8, is very similar to our vegetable nail in form and color. Like our plant, it is composed of a ball or head, a cup, or paunch, and a stem or root that is planted in the sand. The head is round, and is set in a swollen collar or cup, that ends in a broad paunch that is connected to a very short transparent stem. The head or ball is dark and the paunch is a reddish yellow or chestnut color. Backer thinks the little ball is the fruit or the seed of the plant.[4] However, as we shall see later, the little ball does not seem to me to be the seed of the plant.

[4] Translator's note: Apparently a species of Pilobolus.

Perhaps one might also consider the eggs of the red rust as just so many real ovoid plants, since, after all, each disease is a real rust of the same plant. But merely their simple egg or ball shape certainly is not sufficient proof that they are plants; besides, I do not know whether plants of such a character have been observed in nature. The works of many writers, even during the century of the Real Philosophy, are, alas, often full of dreams and impossible hypotheses because of the lack of correct observations. The scientific observer must convey to posterity only those facts proven by experiment, the sole teacher of every natural truth. The more philosophers examine substances, the more the argument of analogy proves itself to be fallacious, and for this reason, it will always seem suspicious to true observers of nature. It is certain that no one who has not observed the eggs of the red rust with a microscope could ever persuade a real observer to believe they were real plants because of the absence of suckers or absorbing papillae, of stem or foot with which to attach themselves, and because they are scattered at random on the leaves of grain even where there are no ruptured vessels or wounds from which they could obtain nourishment. All these reasons militate against the person who advances the opinion that the eggs are seeds, fruits, or other parts of the parasitic plant of the grain rust and who has no positive observations to prove it. What, then, are the eggs if they are not of vegetable substance? If experience is lacking they can be anything since their nature is unknown, but, when their nature is unknown, the philosopher is silent. It is necessary to have positive and illuminating proof to say that a body is of a certain nature and has certain properties; otherwise, the researches of men are nothing more than pure imagination and the probability that a body is what we imagine it to be, is very slight. The person who accidentally formulates a truth without observations must be called more fortunate than wise and he should never believe himself to be the true discoverer. The person who first demonstrates a natural truth is the author and not the man who states it; otherwise, Democrites, not Galileo, would be the discoverer of the satellites of Jupiter, and the learned Adanson is right in recognizing Jussieu as the only discoverer of the *Polypi,* even

though Imperati and Peyssonel, long before his time, judged them the work of insects.

I have not overlooked a single precaution in order to discover the nature of the eggs of the reddish yellow rust, since such an investigation is absolutely necessary for a full understanding of this pernicious disease of the grain. I must frankly confess that I did a great deal of work before proving to myself with certainty what the nature of those minute eggs is. The observation is most difficult and a thousand things combine to deceive the observer. Some of my friends who aided in the observations believed that the eggs were the caps or heads broken off from the cups of the plants of the black rust, being deceived by the apparent similarity of the heads to the little eggs. But such a resemblance can deceive only one who is satisfied by simple observation and one who does not consult nature with the proper diligence and is not very well acquainted with the use of the microscope. It is certain that the eggs are never found in those places where the groves of the parasitic plants are perfectly black and it would be like saying (for anyone who confuses the eggs with the heads of our plants) that citrons and not oranges belong on orange trees in spite of the fact that the citrons are never found where the orange trees are. Even on careful examination the parasitic plants of the black rust are never found with the eggs, when the latter are collected from the reddish yellow spots in which the color is equal and uniform throughout. If those bodies were really the heads of those plants, it is certain that parasitic plants without heads would surely be found among the infinite number of eggs. With the point of a needle I have removed a great number of those nail-like plants from the leaves of grain that had been allowed to soak in water for a long time and I have gathered a great quantity of red eggs without ever finding any cups without heads. It is true that among a great number of plants of the black rust I once observed one without a head near which an egg lay; but an observation of this kind is in the final analysis one of the rarest; and, then, too, the egg was not similar to the head of our vegetable nails but resembled more an egg of the red rust. This egg probably was on the slide on which

I previously had had red rust, although I thought the slide was cleaned thoroughly. It is also certain that the eggs of the red rust all resemble each other in shape and size, which is not the case with the heads of the nails. The bodies of the red rust are all ovoid, and there is very rarely any irregularity among them, while those of the black rust, as can be seen from figures 1 and 2, are very different from one another when observed closely. The bodies in the latter are ovoid, conical, spherical, and of other various figures and shapes. Such an observation is quite convincing but is not the only one that is so. I have tried a thousand times to remove the heads from the cup both with a cutting needle and by crushing the entire plants between two glass slides but always to no avail, since the few times that the head appeared separated, it was always ruptured; I also tried to separate them by soaking the plants for a long time in water, but I found that the cup and stem, instead of being detached, was broken up and torn, and the head was entirely crushed without becoming at all detached from the cup. It is true that eggs of the red rust have often been observed among plants of the black rust, and the plants of the latter among the eggs of the former; but this only proves that there are red spots in the midst of the black spots and black spots in the midst of the red ones, which are noticeable to the naked eye; such spots, being made up partly of black rust and partly of red rust, just as bodies of a different nature are found among the most crowded groves of higher plants. But what are these little eggs if they are not the heads of our nail-like plants? The following is what I have observed regarding the nature of the eggs, which for some time was quite unknown to me and which I almost despaired of ever discovering, and the more I observed this powder with the microscope, the less it seemed to me to be a real plant or a part of some plant. Such a regular rounded body appeared to me to belong to anything other than the vegetable kingdom and, even though it did belong there, one might perhaps feel that it was a humor that diffused out of the stalk or leaves of the grain and dried and hardened on them, or else some humor deposited there by the air, as so many philosophers had believed to be the case in the grain rust. But I was dissuaded more and more

from believing them to be organic plant bodies because I could never find stems or roots with which they could attach themselves to the plant, or papillae or suckers with which they could extract the juice, nor could I find any signs of rupture. There was added to all this the fact that many times I found the oval granules scattered here and there, where certainly neither scar nor rupture of the vessels was apparent, situated on the longitudinal axis of the plant. On examining the largest red spots more closely I found the bodies crowded and piled on one another. In some cases these were seen situated on very long transparent threads that were intertwined among each other in a thousand ways so that usually I was unable to perceive either beginning or end of them, as can be seen in figure 5.[5]

I had accidentally seen in the course of my observation of the red rust a few eggs here and there, attached to an appendage or stem; but I found only three of these among the many millions I examined, and these were so much more irregular and so very different from the stems found on the plants of the black rust that I believed them to be freaks or rather degenerates of the same plants. All this nevertheless made me more eager to observe the eggs. And, in fact, one day while examining a large quantity of the rust I had obtained from many very red spots on which there were no black rust plants, I discovered, with an excellent lens, a perfect egg with a beautiful stem or transparent appendage as may be seen in figure 4, z. This appendage increased in size toward its extremity, where it was attached to the egg. The perfect egg-shape and the very regular stem made me suspect that there was another parasitic plant different from our nails and that the eggs were the fruits or pericarps of this plant. I was all the more confirmed in this opinion because I thought I understood now why these eggs were found without stems or any sign of attachment. The very slender stem must easily be broken off at the slightest shock from its point of attachment to the eggs at which point it is very thin and fragile; hence I understood why the eggs were found without stems

[5] Translator's note: Fontana here probably mistook the vertical walls of the epidermal cells for threads of the fungus.

or visible signs of attachment and were found on the different parts of the leaves or stalk of the grain, carried by the wind to those places where there were no scars or humor to nourish them. These reflections made it seem even more probable that the red dust also was composed of plants; this, however, being only a suspicion which lacked more certain evidence to establish it as a fact. In order to prove that hypothesis, it was necessary to determine just how the eggs of the red rust were attached to the leaves and stalk of the grain plant. If these were heads of parasitic plants, like the caps of the plants in the black rust, and if the form shown in figure 4, z, is a real plant, we should find that the eggs in the red spots are standing erect and not lying down, since the stem is attached lengthwise to the egg in figure 4, z. Although such an observation is very difficult to make, since the dark spots of the red dust are very difficult to illuminate properly, I succeeded after many trials in assuring myself without fear of error that the eggs all stood erect and were closely pressed together. I used a very accurate lens, mounted between two fine silver wires fixed at the center of a very bright silver mirror. Thus assured that the position of the eggs found in the red rust spots was such as to verify the hypothesis that these were real plants, there was necessary only one more decisive observation, similar to that I succeeded in making in the case of the black rust. But try as I might, I was never able to determine how the stems, appendages, or roots of those eggs of the red rust were attached to the leaves or stalks of the grain plant. I examined the red spots in a million ways, with every kind of lens, at all angles either at perpendicular or at oblique angles, with strong and weak light, with reflected or refracted light, but all in vain. I was never able to see a single root in this manner. The eggs were so closely pressed together that it was impossible to see their stems by any means. Almost despairing of success, I finally devised a new method of observation. With the aid of a very sharp needle, I gathered the very fine threads or capillary fibrils from the spots of red rust that were on the stalks and leaves of the grain and tried to the best of my ability not to break off the granules that covered them. I examined these threads both on the glass and when suspended

in the air by a fine steel wire. The examination of most of these threads confirmed more and more my opinion that the eggs of the red rust were attached to very slender stems and were parasitic plants, completely different, however, from those of the black rust.

Finally, I observed one, shown in figure 6, which clearly showed that the eggs were the heads or fruits of very fine filaments attached to the stalk of the grain plant. The filaments of the eggs were very slender and transparent and they appeared to thicken at the extremity farthest from the egg. Assured by this observation, which showed that the eggs rested on filaments attached to the grain plant, I wanted to determine further whether or not they were plants growing perpendicularly to their resting position. I cut with a razor the portions of the leaves or stalk where the red rust appeared thickest and thinned them by removing as many strands of tissue as possible so as to make them transparent to the eye. I passed the point of a sharp cutting needle longitudinally through the spots until they appeared to the eye divided into very slender threads. This bunch of red threads was united at one end and was held in the air by a steel wire; in this way I could observe many threads at one time without further preparation. One of these threads, shown in figure 7, shows the characteristics that make it evident that, like the black rust, it is made up of plants. Growing out of the edges of the thread a very crowded grove of translucent filaments could be seen whose tips were attached to the red eggs. These fine threads were situated perpendicularly to the fiber of the grain plant, similar to the stems of the black-rust plants. In all the infinitely numerous examinations of the red filaments, I always observed that the eggs were attached to slender stems, which were always perpendicular to the thread itself. These stems are incomparably more slender than those of the black rust, and do not have the cup or body that holds the eggs; the threads are very fine and barely visible, even with the aid of the finest lens. In many cases entire thickets or groves of stems without heads were found, a situation never found in the black rust. I have noticed many times that the slightest movement made with the tip of a hair is capable of

breaking off immediately the egg from its stem. Now one can understand why we find eggs without stems in so many places on the grain plant, stems without eggs, and why we do not find the cups of the black rust without heads. The eggs have weak attachments, while the heads of the nails have strong ones, and are furthermore held firmly in their cups, which are of a hard, resistant, and corneous nature.

One can understand also the reason for the difference in color between the black and red rust. This is not due to the somewhat darker color of the heads of the vegetable nails but is due principally to the fact that light cannot pass between the nails because of the large size of their cups vertically disposed on the stalk, nor can light penetrate the heads and cups when they are observed longitudinally as it can when they are few, isolated, and observed transversely.

Assured of the real vegetable nature of the two parasitic plants of the two kinds of rust, the next problem to determine was whether or not these plants had flowers, fruits, seeds, or buds like perfect plants or whether they do not have those bodies, as many botanists claim is the case in the "byssi," many *Fucaceae,* and many species of fungi, which they, the botanists, call imperfect plants. In spite of all the efforts I have made, I must confess that I have been unable to determine whether the plants of the black rust have real flowers, seeds, or buds. It is true that these plants are so minute and become adults so rapidly, that it is very difficult to determine this point either because they are not easily visible even with a microscope or because I have not looked at them under glass at the proper stage of maturity. The latter reason is more significant than the former. I have examined those little plants a thousand times and have never observed anything that resembled or that I could imagine were flowers or real seeds.

Although the rust plants are very small, probably the smallest in the world, I am sure I would have seen the flowers under the microscope, had they been present, even though they were a thousand times smaller than the plants shown in fig-

ures 1 and 6. With all this I have not the intention to conclude anything or deny anything where more positive observations are lacking. Botanists never suspected the presence of stamens in imperfect plants until Micheli discovered them, even though, for the most part, he had himself taken the buds for male organs. The stamens of *Pilularia* and of *Lemma,* discovered by Jussieu for the first time, and in other ferns by Maratti; and many other minute plant parts, never even suspected, were discovered after the microscope came into use. The seeds of some algae, of mosses, fungi, and of ferns have been discovered in our times; and who knows whether a better-oculated observer might not still discover those belonging to the plants of the black rust. I wish to point out one observation that might lead us to suspect that those plants have seeds, which are enclosed in the heads or acorns. I have seen what appeared to be very minute ovoid bodies come out of the acorn or head many times on breaking or rupturing the plants with a sharp needle or between two glass slides.

There have been workers with whom I have come in contact in the course of my experiments who suspected that the eggs of the red rust and the heads of the black rust were the seeds of the two species of parasitic plants. Such an opinion does not appear in any way defensible. First, the large, in fact, enormous, size of those bodies in comparison to the plants, makes this hypothesis quite improbable. The eggs of the red rust are hundreds of times larger than their trunks or appendages, and the heads of the black rust are half as large as the cup and the stem together. Secondly, the heads of the black rust are attached so firmly in the stalk or cup that it is quite impossible for the wind or any other agent, as observations and facts necessarily require, to break them off and disseminate them for germination. I have kept for more than a year, many of the parasitic plants of the black rust, collected at different stages of maturity, and have found it very difficult, even after all that time, to separate the heads from the cups. Thirdly, those plants would have only one seed each, and because of this, would become extinct in a very short time, since not all seeds produce a plant, many of them being sterile and a large number being unable to find a place

to root. Nor can that hypothesis explain why the rust spread so rapidly and to such an extent as to occupy so many provinces at one time as it did in Italy last year. A single seed per plant would not account for their great spread, and besides, one would have to assume that in the preceding year the rust had been as prevalent as in the following year, which is truly absurd.

It is much more probable that those bodies are the fruits or pericarps of the rust plants. We have a thousand plants that bear their pericarps on the tops of the stem or stalk, as in the various species of molds, which are analagous to these. I must confess that with all the care I have used, I have never been able to observe those balls inside the heads of the black rust plants similar to those first discovered by the well-oculated Micheli in the mold plants. Not only have I never been able to see any one of these which had opened by itself naturally, as can normally be seen in the higher plants nor have I ever been able to see any of the red-rust eggs which had opened by themselves. But all these things do not, in truth, prove that the heads of the black rust plants are not the pericarps of our parasitic plants. However, I cannot assume much concerning the nature of the real seeds of this plant without fear of erring. It is evident that they are never seen with certainty, but, in the final analysis, their imperceptible minuteness could render them invisible, even with the aid of the most delicate and accurate lens.

But it has been demonstrated that all plants are propagated either by seeds or by organic vegetable matter! And how absurd it would sound to say that they were propagated by some other means. Our microscopic grain rust plants are of the simplest, i. e., lacking totally canals and organs and one could almost think them formed of a pure inorganic gelatinous juice; and perhaps, because of their simplicity, they are the polipi of the vegetable kingdom. Dillenio, the great botanist and observer, who, more than any other, has examined the microscopic imperfect plants, does not believe it absurd to maintain that such plants, which form the connecting link between the vegetable and the mineral kingdoms, may

have the same origin as stones and minerals, which certainly do not have real seeds.

Although positive observations are lacking to justify the conclusion that real seeds are present in the plants of the black rust, we have many observations that lead us to believe that the eggs of the red rust are the fruits, pericarps or receptacles of real seeds. I have succeeded at various times in opening some of these minute eggs with a sharp needle and have found them filled with small, ovoid and slightly transparent bodies. This is even better observed when the small bodies are lightly crushed between two thin glass slides. In order to observe them properly it is necessary also to add a small drop of water between the two slides and to apply only a slight pressure, since too much pressure will crush all the material and only an uniformly homogeneous, gelatinous mass will be seen. When my technique was successful, I have been able to count as many as 20 of those ovoid granules, that had issued from the eggs, floating freely about in the water. The egg appears crushed but its membrane is not very fine nor very transparent. This observation is very difficult and is rarely successful even when the greatest possible precautions have been taken. We see, then, there is some foundation for the belief that the eggs of the red rust plants are real pericarps containing seeds, the latter being the above-mentioned oviform granules, which can be seen inside the egg. These observations lead us to hope that we shall some day also establish some more certain facts about the seeds of the black rust plants.

However that may be, it is certain that the black rust, as well as the red rust, of grain consists of real plants, even though they must probably be placed in the imperfect group, so called by botanists because of the lack of many parts found in the more common plants. But lack of knowledge concerning those parts cannot hide or destroy certain facts. M. Guettard, in an excellent memoir included in the Proceedings of the Paris Academy of Science, 1756, separates parasitic plants into two groups, the true and the false. He called false plants those that attach themselves to others, forming ulcers in which

they live without causing damage and offense to the principal plant; for example fungi, lichens, and many other plants that climb or grow on the (host) plants. The true parasite is the one that attaches itself to healthy and undamaged plants, which forms lesions and wounds on them by introducing their roots or absorbing papillae into them and living at their expense; for example *Cuscuta, Clandestina, Ipocistide, Orobanche,* and the Orobancoid-like mistletoe. It appears that our rust plants should be placed between these two types of parasites, since it seems certain, on the one hand, that they attach themselves to the grain plant only when there is a rupturing of the vessels and a diffusing of the humors, and that, on the other hand, they feed or nourish themselves at the expense of the stalk or leaves of the grain plant; thus they would be called semi-parasites.

Since we have concluded and established the fact that the grain rust is a mass of small parasitic plants, found in incalculable numbers, it is easy to understand the effects produced on the growing, immature grain. First of all, if the stalk and the leaves are attacked by this terrible malady, the best set (of kernels), promising a heavy yield, is reduced to nothing or almost nothing because such a great number of greedy and gluttonous plants absorb nearly all the nutritive humor of the grain, causing it to become wasted and consumed because of the loss of the nourishing chyle.

Provided that the weather favors vegetative growth, the disease will not usually be serious if the rust attacks the young grain before it has set its stalk. In this case the parasitic plants suck the humor from the leaves only; the grain can set new sprouts, recover, and well nourish its kernels when the stems are not damaged or vitiated.

If, on a vigorous and extensive rust outbreak, there falls suddenly a heavy rain, which wets and washes thoroughly the infected stalk and grain leaves, the rust will almost totally disappear, and the grain will suffer very little damage. The reason for this is very clear. The sudden rain, pouring upon it on all sides, causes them (the eggs) to break from their

stalks and scatters them before they are hardly able to take root, or before they had enough time to suck very much nutriment juice from the grain.

The kernels will be empty and will consist only of pericarps when harvested, if they are attacked by the rust when still tender and milky. The kernels will be only slightly thinner and lighter if the rust attacks the plant when it is nearly mature and the kernels have acquired some consistency and already have a dense, hardened humor. In the first case, where the parasitic plant absorbs the nutrient humor of the grain, the spike remains improverished for which reason it cannot nourish itself and grow or develop the proper starch content. In the second case the grain is already formed with a hard pulp and the loss of the humor taken from it makes it lighter but cannot make it void of substance.

It is now clearly understood how the rust can cause so much damage in such a short while at the time of maturity. We are dealing with a great number of hungry and insatiable plants that live by violence, feeding at the expense of the tender green plant; they grow rapidly, thanks to the food that they steal from the grain, feeding in a great number of places, stopping entirely the flow of the already prepared and digested juice, which is to nourish the grain and to be converted into pulp and flour. By observation, it is a fact known to all observers that the parasitic plants are able to cause comparatively much larger plants to become ill by sucking from them quantities of humor much larger, in comparison, than the parasitic plants themselves.

Modern observers have noticed that the rust of grain is most prevalent when heavy dry fogs are followed by a hot sun and cold nights. The nutrient juice, held during the night in the very thin canals near the epidermis of the plant, is heated by the hot sun in the daytime, causing it to become turbid, altered, and fermented, which, in turn, causes the very delicate vessels to break and the altered humor to exude. The rust spots of the disease are always found on the longitudinal axis of the grain plant and never on the transverse axis, since the fibers and the vessels are broken by the lesser re-

sistance and open more easily on the longitudinal axis. The seeds or buds of the parasitic plant, with which the air is always filled to abundance, as it is with seeds or buds of a thousand other microscopic plants, such as molds, lichens, fungi, etc., fall into these wounds or apertures, attach themselves and take root in grooves, where they come in contact with the exuded humor that is detrimental (to the grain) but at the same time suitable to fertilize and nourish the rust plants that develop with incredible rapidity in this medium.

I must not overlook mentioning an accidental observation I made which may prove very useful and may greatly diminish the amount of spoilage and ruin by the rust in times of famine. From a farm near the city I collected several grain plants that were still fresh and had become infected by the rust only a short time previously; I was careful in selecting only those that were seriously attacked and whose leaves and stalks were almost completely covered with a thick coat of the rust. The kernels of the spikes were greenish and tender and gave forth a milky humor when broken. All the parts of the plant dried up very quickly, since the season was very warm. I found the kernels hardened and resistant even to heavy pounding. They were somewhat lighter, thinner, and a little more wrinkled than healthy, mature kernels, but they contained white pulp of optimum quality, which could be used for bread. Such an observation caused me to wonder just how much grain had afterward been harvested from that farm; to my surprise, I learned that they had not harvested a tenth of the original amount of seed sown. I wrote to various people regarding this and found that many farmers in Tuscany, Bologna, and Romagna, who cut their grain at the first signs of rust, even while it was still green, had obtained yields that, although mediocre, were incomparably higher than the yields of neighbors who, under similar circumstances, had waited and had cut their grain at the regular harvest time. This important observation, which is as yet known to very few, should be followed up by an intelligent observer to determine the value of such a practice. The problem to be solved may be briefly stated as follows: "To establish and demonstrate whether it is practical to cut the grain while it

is still immature when you have a given quantity of rust infecting a province and a given degree of maturity of the grain." Surely, any one skilled in agriculture can readily see the value of such an investigation and can see the great difficulty of fixing precisely all the circumstances, so that such research may be useful and positive. It does not suffice to say, in general, that such a practice may be useful; we must determine when and to what extent it may be of value. In order to accomplish this the eye and head of a sagacious observer are necessary.

The reason for this unexpected observation may be easily explained; it is perhaps inherent in the very nature of the rust. The sun quickly hardens and fixes the milky juice of the kernel in the cut plant, which is unable to take up new humors, causing the water of succulent parts to evaporate and the cortex to harden and dry up, which, in turn, prevents the major part (of the humor) from leaving the kernels; in this way the kernels remain full and pulpy, though somewhat lighter. In the still green and uncut rusted plant, which is not prevented from absorbing the watery vapors during the night, the milky humor remains more dilute and watery. Since this humor is unable to mix with the other more dense and viscous juices, the real blood of the plants, it is stopped and absorbed in every part of the plant by the innumerable small plants composing the rust. The sun then compels the excessively fluid humor to leave the plant by transpiration, which causes the kernels to become impoverished and empty. The milky humor of the kernels may descend through the canals of the stalk, in that circulation which is admitted by all physicists to exist in plants, and, encounters everywhere the parasitic plants, which attract and suck it up, since, even in the case of the animal body, the humor flows most freely where it finds the vessels broken and meets with less resistance; thus it is also possible that the thin and fluid humor of the kernels still may be carried in large quantities to the wounds made in the stem where it affords nourishment to the enemy plants. However it may be, it appears that the facts are certain, and the problem, indeed, merits the attention of one who is interested in the welfare of society.

A long and well-understood series of observations on the rust of grain, made by an intelligent observer, might bring out more important lights not only on the nature of the disease but, also, on the nature and economy of the parasitic plants that produce it. Above all, it is necessary to observe the beginning of the disease, to examine closely the condition of the plant, the germination and development of the parasitic plant, and, finally, the real nature, the origin and causes of the rust. It will then be less difficult to find remedies that will enable us to exclude from our wheat fields such a terrible scourge, which impoverishes and starves us.

"If we had," says Duhamel, "sufficiently accurate knowledge of the causes of the rust, we should probably succeed in finding more easily protective agents against this disease; but, meanwhile, it is very profitable to assemble all the observations made on this disease by the lovers of Agriculture."

The talents of many learned botanists could be used to greater advantage in the little-known fields of the vegetable kingdom, if, instead of furiously pursuing new systems and enriching with new barbarous words one of the most delightful and perhaps the most useful branches of the science of nature, they observed the structure of plants more closely by examining their nature and economy more in detail, especially in the case of the so-called imperfect plants, which can be properly observed only with the microscope, and concerning which confusion, uncertainty, and obscurity reign even after Micheli and Dillenio. It is true that there have been very few botanists who have occupied themselves with the more exacting and tedious details of these minute plants; and among these we must recognize Micheli who found the male organs and the seeds of the fungi, Dillenio who first observed the seeds of the mosses, and, finally, Bernard Jussieu who gave to the science of Botany the stamens and the seeds of ferns, the exploding of dust in fertilization, and the true origin of the corals, the madrepore, the lithophytes, serophytes, and many other marine plants. But these men were very great and exact observers, and only observations guided by the spirit of analysis can enrich us with important new truths. The physicists could also turn their researches towards plants,

and from their united labors we might hope for almost anything. The vegetable kingdom presents a vast field in which observers can usefully occupy themselves; the structure and economy of the plants are the only physical science of those organized vegetable bodies; a vast and difficult study, worthy of occupying the mind of any philosopher, both for the great value that it can bring to society and for the inward pleasure, little understood by the multitude, that is gained when one penetrates into the hidden secrets of the delicate mechanism animating and governing those regular and highly complex bodies. But what does not the vegetable kingdom owe to a Hales, a Bonnet, or a Duhamel in these latter times? The difficult discovery of the male organs and the seeds of the *Fucaceae* must be credited in the final analysis to the very eminent French physicist, Réaumur.

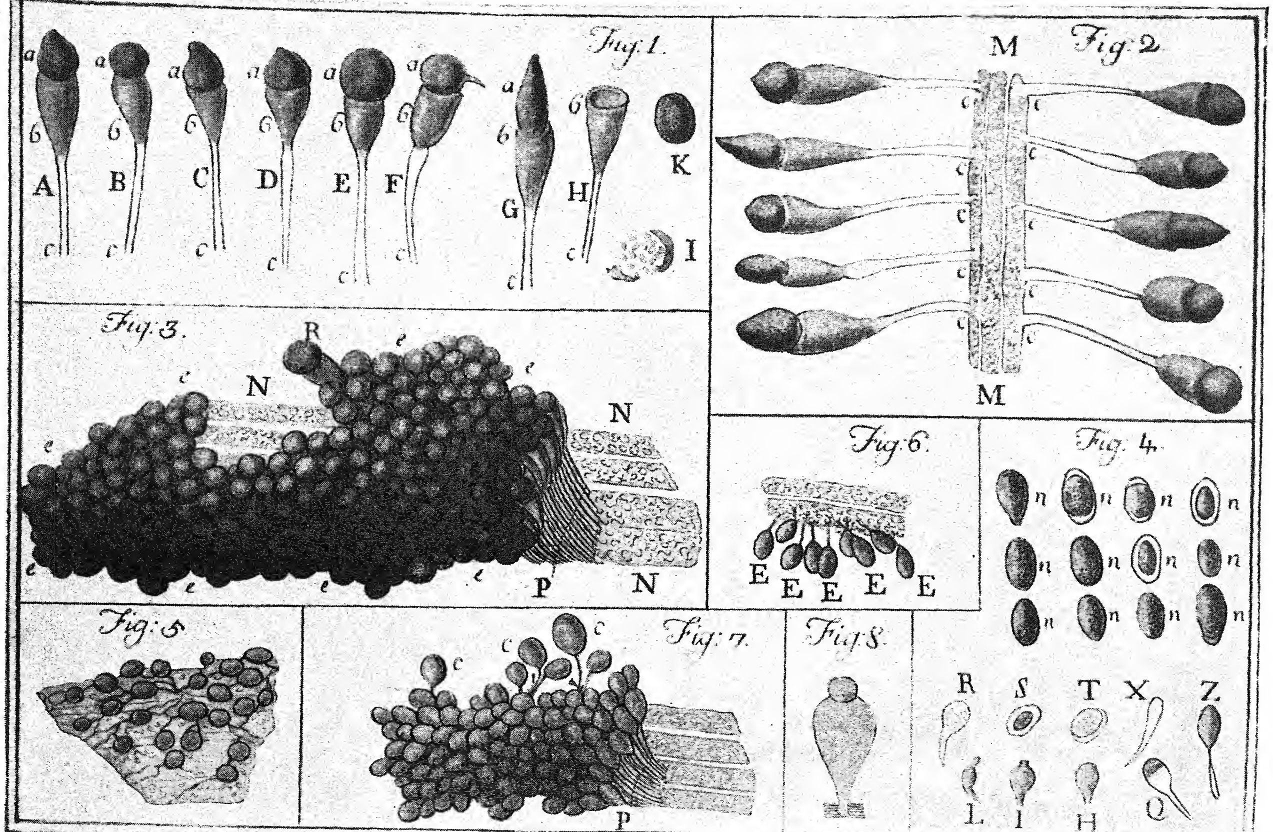

Fig. 1.
A
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Fig. 2
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M
Fig. 3.
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Fig. 6.
E E E E E
Fig. 4.
Fig. 5
Fig. 7.
P
Fig. 8.
R
S
T
X
Z
L
I
H
O

EXPLANATION OF THE FIGURES [6]

Figure 1

A, B, C, D, E, F, G, represent seven parasitic plants of the black rust observed on a glass slide with a very strong lens.

a. a. a. a. a. a. a. The heads or caps of those seven plants, the heads are of different shapes and sizes; some are spherical as may be seen in plants B and E; others are olive-shaped as in G; and others still different as in A and D.

The very round head in F carries a curved cone, which is very rarely observed.

b. b. b. b. b. b. b. The bodies or cups of the plants of various sizes and shapes, with circular edges, more or less raised, at the tips of which the heads are planted and in which they are set, although always in different positions and manners.

c. c. c. c. c. c. c. Transparent stems of variable diameter, which end in and are tied to the cup.

H. A cup with the stem or appendage, but without the head, and the top hollowed, making a little depression.

K. A very large egg, exactly similar to those found in the reddish yellow rust, observed on the glass only once in the immediate vicinity of H.

I. An egg that was crushed between two slides and observed with a very strong lens; many extremely small bodies are seen coming out, the same oviform bodies appear to be discernible in the head of plant, E.

Figure 2

M. M. A small section of the stalk of a grain plant observed on a glass slide.

c. c. c. c. c. c. c. c. c. c. The appendages or stems of ten plants inserted in the above-mentioned stalk as appears in the same figure.

Figure 3

N. N. N. A piece of grain stalk.

e. e. e. e. e. e. e. e. A rust spot observed with a powerful lens. In those spots all the heads of the small plants may be seen touching each other and their stems are disposed perpendicularly on the stalk.

P. A mass of appendages or stems of those parasitic plants rooted in the stalk and leaning on one another.

R. One of those plants separated and slightly raised above the others.

Figure 4

n. n. n. n. n. n. n. n. n. n. n. n. The eggs of the red rust observed with a very powerful lens and represented with their least ordinary

[6] Figures 1 to 8, as published in the original text, were done in natural color.

differences. They are generally all pointed and some appear surrounded by a transparent roll (or cake); others are seen with a ring resembling the ring of Saturn.

Q, R, S, T, X, Z. Very rare bodies of the reddish yellow rust, although some of those that have stems are found in the black rust.

Q. A body with a stem ending in a point. The stem and half of the head are transparent; and the other half is reddish yellow.

R. Another body with stem, entirely transparent, with a few dark spots in the acuminate head.

S. An egg with a wide roll around it; in its center another opaque egg, reddish brown.

T. An irregularly round, totally transparent body.

X. A very long thin body with a stem, entirely transparent.

L, I, H. Three eggs of the reddish yellow rust, crushed between two glass slides.

L. An egg with two large handles, slightly curved, which project from the two extremities.

I. Another egg with straight handles.

H. A third egg with only one handle.

FIGURE 5

A piece of the stalk of a grain plant with many filaments laden with eggs of the red rust.

FIGURE 6

E. E. E. E. E. Some plants of the red rust attached by their stems on a fibril of a grain leaf.

FIGURE 7

A red rust spot.

P. The stems of these plants, which stand perpendicularly on a piece of the stalk of a grain plant.

c. c. c. Separate plants with filaments or stems, to which the eggs of the red rust are attached.

FIGURE 8

A microscopic plant observed by Backer in the moist sand of the Thames.

Phytopathological Classics

NUMBER 3

THE DISCOVERY OF BORDEAUX MIXTURE

Three papers

I. Treatment of Mildew and Rot.
II. Treatment of Mildew with Copper Sulphate and Lime Mixture.
III. Concerning the History of the Treatment of Mildew with Copper Sulphate.

BY

PIERRE MARIE ALEXIS MILLARDET

1885

A Translation by

FELIX JOHN SCHNEIDERHAN

of

Traitement du Mildiou et du Rot. Journ. d'Agr. Pratique, **2:** 513-516. 1885

Traitement du Mildiou par le Mélange de Sulphate de Cuivre et de Chaux. Journ. d'Agr. Pratique, **2:** 707-710. 1885.

Sur l'Histoire du Traitement du Mildiou par le Sulfate de Cuivre. Journ. d'Agr. Pratique, **2:** 801-805. 1885.

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PIERRE MARIE ALEXIS MILLARDET

Pierre Marie Alexis Millardet was born December 13, 1838, at Montmerey-la-ville in the Department of Jura, and died December 15, 1902. He came of intellectual parents and he was thoroughly educated. He first studied medicine but gave up a good practice to study botany, first under Hofmeister at Heidelberg and, later, under de Bary at Freiburg. He was not only a profound student of botany but also an excellent artist, his drawings being the admiration and envy of his fellow students. Upon returning to France, he took the doctorate in both medicine and science. He held the positions of assistant professor of botany in the University of Strassburg in 1869, professor of botany at Nancy, 1872, and finally, professor of botany at Bordeaux in 1876, where he remained until his retirement in 1899.

His first work was entirely in pure science but his later years were devoted to economic botany. The introduction of the Phylloxera and the downy mildew fungus, *Plasmopara viticola*, from America, caused Millardet to devote all of his efforts to applied botany. Millardet and his colleague, Planchon, found mildew at about the same time, but in different parts of France in 1878. The introduction, by Millardet, of resistant American vines as stocks for grafting of the European varieties, saved the French vineyards from damage by Phylloxera. His observation of the prophylactic effects against mildew of a copper sulphate-lime mixture used to sprinkle grapevines along the highways to prevent stealing of the grapes, led to the discovery and development of Bordeaux mixture. The discovery of this famous fungicide undoubtedly saved the wine-growing industry of France and is probably Millardet's greatest contribution to science.

F. J. Schneiderhan

1838-1902

TRANSLATOR'S FOREWORD

The translation of several of Millardet's classic articles about the origin of the copper sulphate and lime mixture, (Bordeaux mixture), as a fungicide, was undertaken in connection with the review of literature for the translator's doctor's dissertation at West Virginia University. The translations were made from photostatic prints of the original articles appearing in the *Journal d'Agriculture Pratique,* found in the library of the United States Department of Agriculture.

It has been the custom, from time immemorial, to sprinkle grape vines along the highways of Médoc, France, with a thick mixture of copper sulphate, lime, and water to prevent thieves from stealing the grapes. Mildew was first observed in France by Millardet in 1878. In October, 1882, he noted that this copper sulphate and lime mixture, used to repel thieves, also controlled mildew. This famous observation is generally considered to have been an accident. May it not have been an incident in the daily work of such an astute observer as Millardet? Be it accident or incident, such was the discovery of Bordeaux mixture.

This discovery is probably as momentous as any event in the whole history of our efforts to control plant pathogens. This fungicide was the first to be used on a large scale the world over. The rapidity of its adoption and general use wherever certain diseases of fruits and vegetables occur, indicates how economically fortunate was Millardet's observation along the highways of Médoc.

F. J. Schneiderhan

TREATMENT OF MILDEW AND ROT

Traitement du Mildiou et du Rot. *Journ. d'Agr. Pratique,* **2**: 513-516. 1885.

Since the appearance of mildew in France in 1878, I have not ceased to study *Peronospora* in the hope of discovering in its development some vulnerable point that might permit of its mastery. The results of my observations are set forth in various publications.[1]

I had noticed in the course of my researches that the summer spores or conidia of *Peronospora* easily lose their germinating power. This observation and the failure of all the treatments attempted until then had led me to formulate this conclusion,—*that a practical treatment of mildew ought to have for its objective not the killing of the parasite in the leaves infected by it, which seems impossible without killing the leaves themselves, but of preventing its development by covering, preventively, the surface of the leaves with various substances capable of making the spores lose their vitality or, at least, of impeding their germination.*

Three years ago, therefore, I was in search of a substance that would satisfy my concept, when chance put it into my hands.

Towards the end of October, 1882, I had occasion to traverse the vineyard of Saint-Julien en Médoc. I was not a little surprised to see, all along my way, that the vines still bore their leaves, while everywhere else they had long ago fallen. There had been some mildew that year, and my first reaction was to attribute the persistence of the leaves along the way to some treatment that had preserved them from the malady. Examination, indeed, permitted me to confirm immediately the fact that these leaves were covered in great part on the upper surface by a thin coating of a pulverized bluish white substance.[2]

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[1] *Journal d'Agriculture Pratique,* nos. des 10 février 1881, 6 juillet 1882.—*Mémoires de la Société des sciences physiques et naturelles de Bordeaux,* t. V (1884) p. XXIV. —*Zeitschrift für Wein-, Obst-, und Gartenbau für Elsass-Lothringen;* nos. des 1er et 15 mars 1883.

[2] Baron Chatry de la Fosse made the same observation that I did, but two years later, in 1884. It was, in fact, his communication to the Society of Agriculture of la Gironde that, from the first of May, decided me to publish the yet incomplete results of my studies.

After arriving at the Chateau Beaucaillon, I questioned the manager, M. Ernest David, who told me that the custom in Médoc was to cover the leaves with verdegris or with copper sulphate mixed with lime, when the grapes were ripening, in order to keep away thieves who, on seeing these leaves covered with the copperish spots, would not dare to taste the fruit hidden underneath for fear of its having been blemished in the same way.

I called the attention of M. David to the fact of the preservation of the leaves under immediate consideration and shared with him the hope, engendered in my mind, that the salts of copper might form the basis of the treatment of mildew. M. David at first, I must say, made several objections, but finally accepted so completely my ideas and seconded me so efficaciously that it is my duty to ascribe to him the major share in my ultimate success.

The following year (1883) I tested the treatment several times in my garden, either with the material mentioned in the foregoing or with others. M. David repeated the majority of these experiments at Dauzac, in Médoc, on the estate of M. Nathaniel Johnston of which he was manager.

It was the same in 1884 but, by mischance, the mildew was so light in the vineyards where the tests had been conducted that it was impossible to judge accurately of the value of the different treatments that had been applied.

Nevertheless, since there was a certain rumor concerning the treatment by copper sulphate, I decided to share the results obtained with the Society of Agriculture of la Gironde. On the first of May, 1885, I gave the exact composition of the requisite liquid according to M. David's tests and the instructions relative to both the method of application and the most favorable time to apply the treatment.[3]

Following this communication several large landowners of Médoc were not afraid to apply in full the treatment I had recommended.[4] M. N. Johnston, to whom I had confided my ideas in 1882, and who for two years had followed the tests made by M. David, his manager, went into the matter

[3] *Annales de la Société de la Gironde,* 1885, p. 73.

[4] I shall specially cite M. Barton (Chateau Léonville), MM. de Bethmann and Faure (Chat. Gruand-Larose), M. De Ferrand (Chat. Mouton d'Armailhacq), M. Lawton (Chat. de Pez), M. A. Lalande (Chat. Brown-Cantenac), M. Merman (Chat. du Crocq), M. Solberg (Chat. Siran).

in earnest, and he alone had 150,000 vines treated on his two estates at Dauzac and Beaucaillon. The results throughout surpassed my expectations.

Today, the 3rd of October, the treated vines show normal growth. The leaves are healthy and of a beautiful green, the grapes are black and perfectly ripe. The vines that were not treated present, on the contrary, the most wretched appearance, the majority of the leaves have fallen; the few that remain are half dried up; the grapes, still red, will not be fit for anything except sour wine. The contrast is striking. I will add that my colleague, M. Gayon, Professor of Chemistry on the Faculty of Sciences, has been willing to examine the unfermented wines produced by the grapes of the treated and nontreated vines. He found on the same vine (the Malbec):

	Vines treated	*Vines not treated*
Sugar	117 gr. per l.	91 gr. 8 per l.
Acidity produced by sulphuric acid	5 gr. 1 per l.	7 gr. 7 per l.

What enhances still more the value of the experiments of which I am speaking is that they were performed as methodically as possible. In each plat treated there were, as controls, several rows of nontreated vines. I shall point out besides that the treatment was applied preferably to the vines most susceptible to mildew, i.e., the Malbec, the Cabernet franc, and the Petit-Verdot, so that its effects on the less susceptible vines can be only still more satisfactory. Finally, let me add, the disease has this year been exceptionally severe.

All of these considerations, it seems to me, sufficiently warrant my affirming in the most positive manner the efficacy of the treatment, concerning which I speak, against a scourge that, until now, has withstood all efforts in Europe as well as in America; that is to say, mildew, properly speaking, and rot or mildew of grapes. But there is more; the close analogies that exist between the *Peronospora* of the vines and that causing the disease of the potato and tomato cause me to hope that we shall henceforth have at hand a real prophylactic treatment for these latter diseases.

I must now state the nature of the treatment and how and at what time it should be applied.

In 100 litres of water (either well, rain, or river) are dissolved 8 kilos of commercial copper sulphate. Then, from 30 litres of water and 15 kilos of rich rock lime, milk of lime is made and mixed with the solution of copper sulphate. It forms a bluish paste. The workman pours part of the mixture, while stirring it, into a watering pot, which he takes in his left hand, while, with the right, by the aid of a small brush he wets the leaves, taking care constantly not to touch the grapes. One need fear no harm, even to the most tender organs.

At M. Johnston's, 50 litres of the mixture sufficed, on an average, for treatment of 1,000 plants, which, for one *hectare* (10,000 plants), places the total expense (cost of materials and labor) at not more than 50 francs.

The treatment was carried on from the 10th to the 20th of July at several points; the operation was repeated toward the end of August, but without great advantage. It is, therefore, established that a single application is sufficient.

When the mixture dried it adhered well to the leaves. After treatment the vines withstood several storms at the beginning and at the end of August and frequent rains in September. In spite of that, one can still recognize easily today, on more than half of the leaves, the places where they were touched with the mixture. But those that have retained no trace of it are in as good condition as those that are still spotted.

It is not necessary that the leaves be wholly covered by the mixture. I believe it safe to assert that a single spot of it on each leaf is sufficient. This fact it seems to me derives from an examination of the leaves as well as from the comparison of the results obtained from the tests, very carefully executed at M. Solberg's by his manager, M. Feuillerat, with results of treatments performed in an ordinary manner. A mixture of iron sulphate instead of copper sulphate, made by M. Feuillerat, produced good results but was inferior to the copper mixture. Various experiments have proved that copper sulphate alone (at a harmless dose) and lime alone are almost innocuous. The verdigris (500 gr. diluted in 10 litres of water) can, it is true, rival the mixture of copper sulphate and lime but it is much more expensive than the latter.

The experiments of this year show how justified I was in insisting, in my communication of May 1st, to the Society of

Agriculture of la Gironde, upon the necessity of applying the treatment preventively, that is, as soon as the mildew appears in the vineyard. Everybody who has treated vines that were already somewhat seriously affected has realized only the slightest benefit from the operation.

There is a final important item to consider. In spite of all my precautions it happened that a few drops of the mixture fell on the grapes. Will the copper reappear in the wine? And if it be found there, will it be sufficient in quantity to affect the public health?

My colleague, M. Gayon, has been willing to promise me his cooperation to elucidate this question. A preliminary attempt made by him on 800 grams of grapes coming from treated vines has not revealed with absolute certainty any copper. Researches in this direction will continue and I hope in a short time to be in a position to submit the results of them to the wine growers.

MILLARDET
Professor on the Faculty of
Sciences of Bordeaux

TREATMENT OF MILDEW BY THE MIXTURE OF COPPER SULPHATE AND LIME

Traitement du Mildiou par le Mélange de Sulphate de Cuivre et de Chaux. *Jour. d'Agr. Pratique,* **2:** 707-710. 1885.

The salutary effects following the treatment of mildew with the copper sulphate-lime mixture, are accepted today beyond all question. There remains to explain the mode of action of this treatment, why it has been chosen in preference to others, and for what reasons, although probably susceptible to some changes, it appears to me scarcely possible that it will undergo any important modifications.

The observation that led me to conceive the very principle of the treatment dates back several years. I was studying the development of summer spores or conidia of *Peronospora* when I established the fact that the reproductive bodies never developed in water from my well, although when sown in the city water, rain water, or dew and distilled water, they did not delay in completing their evolution by developing zoospores.[1]

The explanation of this strange fact escaped me for some time. It occurred to me only at the very last, as will be seen farther on. As the water from this well is so noticeably impregnated with limestone salts that it cooks vegetables badly, I thought at first that it might have been to this peculiarity that it owed its harmful action on the conidia. But, whatever might have been the cause of this action, I inferred from it that, in any case, *its extremely weak and scarcely discernible effects* were capable of preventing the development of the reproductive bodies of the parasite.

Since, on the other hand, I had taken account of the impossibility of destroying the *Peronospora* without destroying at the same time the leaves stricken by it, for the parasite grows exclusively within these organs until it fructifies, I had arrived at this conclusion, namely, that the treatment for mildew could be only preventive. It should suffice, as I said, to cover preventively the surface of the leaves with divers

[1] This fact appears in the *Annales de la Société d'Agriculture de la Gironde.* 1885, p. 79.

substances capable of causing the summer spores to lose their vitality or, at least, to impede their germination.[2]

I was at that point then, when, in 1882, I witnessed for the first time the favorable action of the mixture of copper sulphate and lime against mildew, a mixture used from time immemorial in Médoc to prevent theft.

It seemed to me that the really active agent in this mixture must have been the copper, in spite of the fact that this metal was in an almost insoluble condition.

So, from the following year, (1883) I devoted myself to uninterrupted research on the efficacy of the different salts of copper (sulphate, carbonate, phosphate, sulphur) and the same salts of iron,[3] as well as on that of the lime in powdered form or mixed in water. These experiments were repeated by M. David of Dauzac at M. Johnston's, who had put his property at my disposal for these experiments, with a generosity for which I can not thank him too much. At the same time, the mixture of copper sulphate was tried in varied doses of the two component materials.[4]

In 1884 the same tests were again performed, but with no definite result, for there was but very little mildew where the tests were made and it was impossible to judge exactly and comparatively the effects resulting from the methods of treatment. However, these two years of research had enabled me, as they had M. David, to recognize the fact that the mixture of copper sulphate and lime was, of all the substances used, the one that had produced the best results. We were induced to give up the iron sulphate and even the copper sulphate treatments, which seem, however, to have given rather good results,[5] because the spraying of these solutions is difficult and they are dangerous to the vine. With a dose of ½ part copper sulphate and 1 part iron sulphate to 100 parts of distilled water, I burned without fail the young shoots and often the leaves wherever the spraying was not very fine and where the liquid collected in drops. Nevertheless, the small quantity of salts deposited on the

[2] *Annales de la Société d'Agriculture de la Gironde, loc. cit.*

[3] I know from my tests that the salts of iron also act favorably against mildew.

[4] All of those tests conducted at Dauzac, with their respective dates, will be found in the diary of the Chateau Dauzac. This important item has been communicated to the commission of the Society of Agriculture of la Gironde for the study of the results of this treatment.

[5] Especially, for MM. Ad. Perrey, P. de Lafitte, Magnien.

leaves by this process appeared to me insignificant in consideration of the end to be attained. Lime alone, applied as a dust, appeared to be almost innocuous. Milk of lime, in a single application, was scarcely more potent.

It is for these reasons that in 1885 we directed all our efforts—M. David and I—in the direction of the copper sulphate and lime mixture, employing it in doses of the two component materials that, according to the tests of the two preceding years, appeared to be the best. It is, indeed, one of the reasons why nowhere else in Médoc the result of the treatment has been so satisfactory as at Dauzac.

In addition to the foregoing, another method gave us results that confirmed those obtained by our direct observation in the vineyard of the effects of the different treatments by the substances mentioned above.

When one places the conidia of *Peronospora* in pure water, at a temperature above 9°C., an hour or an hour and a half thereafter, they discharge zoospores. These move about in the water from 3 to 5 hours, at first rapidly, then stop and put forth germ tubes.[6] These latter pierce the epidermis of the leaf and so penetrate the tissues that, 6 to 8 hours after the beginning of the experiment, the infection of the leaf by the parasite is consummated.

But if one uses dilute solutions of lime, of copper sulphate, or of iron, one will observe that the conidia and the zoospores produced by them are extremely sensitive to these solutions. If the solution is a little too concentrated for the development of the conidia, the latter do not discharge zoospores, but die without undergoing any noticeable change. If the solution is a little less concentrated, some zoospores develop but, in contact with the solution, instead of moving rapidly, they move about slowly and, without germinating, soon come to rest and die. If, following another step, one sows some conidia in a given quantity of distilled water to which one adds, once the zoospores are in motion, increasing amounts of a titrated solution of lime, or sulphate of iron, or copper, there comes a moment when the zoospores stop and are all killed.

The experiment taught me that the limit of concentration of these different solutions, that is the concentration that is

[6] See my memoir *Mildew and Rot*, in *Zeitschrift für Wein-, Obst-, und Gartenbau, für Elsass-Lothringen*, Numbers March 1 and 15, 1883.

incompatible with the complete development of the reproductive germs, is:

For lime, a solution of	$\frac{1}{10{,}000}$	
For sulphate of iron,	$\frac{1}{100{,}000}$	of iron
For copper sulphate,	$\frac{2 \text{ to } 3}{10{,}000{,}000}$	of copper

That is to say, the salts of iron, although very active, are nearly 100 times less so than those of copper, and lime is 10 times less active than iron.

It is, therefore, understood that in the experiments of the Bellussi brothers, 5 and 6 successive applications of milk of lime were necessary in order to obtain satisfactory results.

Again, it will be found difficult to secure substitutes for salts of copper because of the prodigious energy of their action upon the reproductive germs of the *Peronospora.*

As for the lime, as will subsequently be seen, its rôle in the mixture appears to be no less important.

It is only after having obtained these results that it has been possible for me, thanks to the cooperation of M. Gayon, to give an account of the fact mentioned above, a consequence that has, in fact, been the point of departure of all my researches: I mean the absence of development of the conidia of the parasite when I sowed them in water from my well.

This well is 11 meters deep. The water is lifted by means of an old copper pump. Analysis has disclosed the fact that it contained 5 milligrams of copper per litre, a quantity more than 10 times that necessary to kill the reproductive germs of the *Peronospora.* I should add that I have lived in the house 6 years, during which time the entire family has drunk of this water without the slightest inconvenience. This fact is interesting because M. Gayon will presently state that, in general, the wines produced from the vineyards that have been given the treatment under consideration contain hardly a 16th part of the copper found in the water of which I have just spoken.

Another obscure but very interesting fact, about which I must speak, could be elucidated only by a chemist. It is again to the cooperation of my learned colleague that I owe its explanation.

Copper in the mixture and on the leaves is found as an hydroxide, which is generally regarded as insoluble. Under the microscope it is observed in the form of amorphous granules at first united by the lime and the sulphate of lime, and later protected by a solid and slightly soluble crust of carbonate of lime.

Now, it happens, according to studies conducted by M. Gayon, that this oxide is dissolved slowly but integrally at 15° C. by water containing in solution some carbonate of ammonia; that water containing carbonic acid can dissolve 40 milligrams of the oxide per litre at the same temperature and atmospheric pressure; and, finally, that pure water itself dissolved traces of this same oxide at 15°C.

The little drops of the mixture disseminated over the leaves, therefore, function like true reservoirs of copper oxide, which, for weeks and months, retain the oxide under the protection of their limestone coating and furnish to the water of dew or rain, more or less charged with carbonate of ammonia and carbonic acid, the minimum quantity of the copper necessary to check the development of the conidia that the wind deposits on the surface of the leaves. The lime seems to me, then, to play a triple rôle in the mixture. At the moment of spraying it acts like an energetic mordant which fixes the disinfecting drop on the leaf and establishes its close adherence. For several days, it is capable of killing the conidia and zoospores by its causticity. Finally, when it has been transformed into carbonate, it serves for the preservation of the store of copper oxide.

If the theory I have just presented were in need of proof, one would find it in the following experimental fact, that several persons have this year found out, in Médoc, that the treatment produced its maximum effect there only when it was applied in a preventive manner. As early as the first of last April, I had insisted forcibly upon this important point.[7]

In the issue of last October 8 of this journal, as well as in my communication of the 5th of the same month to the Academy of Sciences, I presented the copper sulphate and lime treatment as a remedy both for mildew and rot. Permit me to present a brief elucidation of this subject.

It was in 1883, in Europe as I have already mentioned, that I first described and illustrated rot in the Alsacian

[7] In the *Annales de la Société d' Agriculture de la Gironde, loc. cit.*

journal cited above. Rot is produced, as I have clearly established, by the development of mildew on the clusters of the bunch and within the grapes. That is, I believe, the most common variety of rot in America and probably the most important for us. Since completing the work of which I have just spoken, I have learned to recognize another kind of rot, which develops subsequent to the anthracnose on the grape clusters of the bunch. Finally, following some recent disclosures, which require confirmation, there seems still to exist a third kind of rot caused by a fungus of the genus *Phoma*.

When I stated in one of my preceding articles that the treatment of mildew likewise prevents the rot, I had in mind only the first kind of rot mentioned above, i.e. that associated with the mildew. It is, in fact, true that, while preventing the development of mildew on the leaves, one at the same time prevents the invasion of the bunch of grapes by the same parasite.

MILLARDET
In collaboration with M. Gayon.

CONCERNING THE HISTORY OF THE TREATMENT OF MILDEW WITH COPPER SULPHATE

Sur l'Histoire du Traitement du Mildiou par le Sulfate de Cuivre. *Journ. d'Agr. Pratique,* **2:** 801-805. 1885.

Thanks to the efforts of a large number of observers and experimenters we now know of an agent capable of preventing the disastrous effects of mildew. It is not only established that the salts of copper, especially the sulphate, constitute a certain preventive treatment of the plague, but we have at hand certain practical methods of administering the treatment. It has interested me to investigate by what steps this discovery occurred and to place the results of the research before the public.

The first mention with which I am acquainted relative to the use of copper sulphate against mildew is found in a work on the development and treatment of mildew and rot published by me on the 15th of March 1883.[1]. In fact, one finds there the following, near the close of the article: "Some recent observations lead me to hope that we may perhaps obtain more satisfactory results through the spraying of certain mineral solutions, sulphate of iron or copper, for instance."

The recent observations to which I refer are those that I made, as I said before, at the end of October 1882, relative to the beneficial effects from spraying with the Médoc mixture.

In the recent work of M. P. Lafitte[2] it is noted that on the 20th of September, 1884, M. Ricaud and M. Paulin published in the *Journal de Beaune* a note on the verification of the good effect of a concentrated solution of copper sulphate on the white wooden props treated the year before in Bourgoyne. M. Ricaud cites the fact, as noticed recently. On the 23rd of the same month and in the same journal, again quoting M. Lafitte, is a note by M. Montoy on the same subject.

Finally, according to the same author, M. Bidaud would have written on the same subject and in the same journal,

[1] *Zeitschrift für Wein-, Obst-, und Gartenbau für Elsass-Lothringer,* numbers for March 1 and 15, 1883.

[2] *Journal d'Agriculture practique,* number for October 1, 1885, p. 479.

current for the month of September but on a date not stated by M. Lafitte.

The original publications in question are unknown to me. But it seems certain to me while writing what we have just read (I have done nothing but copy M. Lafitte word for word) that the author of whom I speak was well informed.

I read, in effect, on page 5 in the notes of the Bulletin of the Committee on Agriculture of the Beaune district, October, 1885, that "The newspapers of Beaune published in support of the efficiency of sulphated props, some articles by MM. Ricaud Paulin, and Montoy in Nos. 20 to 23, September 1884.

These communications were soon followed by one from M. Ad. Perrey to the Academy of Sciences. Beginning with the 29th of the same month, M. Perrey communicated to the Academy the same observations and so discussed them as to place the action of copper sulphate beyond all doubt. His note terminated with the following words "Without wishing to exaggerate the results of an observation that numerous testimonials will without doubt soon confirm, we limit ourselves to introducing the prophylactic agent, counting on the common effort of those interested to hasten the moment when the application will be made general through economic procedure."

With these last words M. Perrey seemed to promise to study, by himself, the action of copper sulphate on mildew. He kept his word, as we shall subsequently see.

Such, to my knowledge, are the first published documents concerning the action exercised by copper sulphate on the deadly parasite. It is fitting and right to accord the honor of it to the Burgundians, in general, and the Committee of Beaune, in particular.[3]

On the third of December, the same year, a large property owner of Médoc, M. le baron Chatry de la Fosse, called to the attention of the Society of Agriculture of la Gironde the salutary effects of a mixture of lime and copper sulphate on mildew. They have the custom in Médoc of

[3] It is, no doubt, worth mentioning here a communication made to *Progrès agricole* of November 4, 1884, by M. P. Estéve, of Montpellier, on a powder devised by him called *Sulfatine*. This powder consists of sulphur, copper sulphate, iron sulphate, and calcium sulphate. According to its inventor, it has for two years been effective against mildew. The exact amounts of the various ingredients that make up *Sulfatine* have not been given out by M. Estéve, who is advertising his discovery.

spraying the vines along the roads with this mixture to keep away thieves. A few weeks later M. de la Fosse imparted his knowledge to his colleagues of the French Society of Agriculture.

The first published communication of any moment that I contributed personally concerning the use of copper sulphate is contained in the Annals of the Society of Agriculture of la Gironde under date of April 1, 1885. This communication was published on the 1st of May in the Journal of Agriculture and Horticulture of la Gironde. Will you permit me to review it hastily in order to show that it represents a notable advance over its predecessors?

In it I stated, in effect, how I had been impressed in October, 1882, that is, two years before M. de la Fosse, by the salutary effects on mildew common in Médoc resulting from the application of copper sulphate sprays to vines and grapes along the highways in order to prevent pilfering. I told of the tests that I, alone, had made in my own garden, or with the help of M. David, manager of the Duchy of Dauzac (Médoc), performed on a large scale relative to the efficacy of different salts of iron and of copper, in 1883 and 1884. Finally, I announced the formula of the mixture, which, after two years of research, seemed to give the best results, and the exact directions as to the time when the treatment should be applied. I shall add that the same explanation of the action of the treatment is, in brief, to be found in this document.[4]

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[4] I shall quote some passages: "Here is how one may prepare the preservative mixture:—In 100 litres of water (well, rain, or river) dissolve 8 kilos of commercial copper sulphate. Then mix with the copper sulphate solution a solution consisting of 30 litres of water and 15 kilos of rich rock lime. It will form a bluish deposit. The workman pours, while stirring, part of the mixture into a bucket or into a sprinkling can, which he takes in his left hand; while, with the right, with the aid of a little brush broom, he sprinkles the leaves with the preservative solution, being careful to prevent its reaching the grapes." I shall note the fact that the treatment ought to be preventive; that it in no way reacts so as to destroy the *Peronospora*, but simply prevents its germ tubes from penetrating the interior of the leaf. Now, the mildew occurs in the vineyards scarcely ever before mid-June. Prior to that time the owners can devote their leisure to the treatment of other pests of the vine. But from that time forward, through the months of July and August, especially, they must be constantly on the watch in order that immediately, from the first evidence of the parasite, the fungicide may be applied within 24 hours, *at least as a preliminary test*. No injury must be permitted to happen, especially to the shoots and the tenderest buds. Such can obtain only if all the copper sulphate is broken down by the milk of lime and enters into a condition of a hydrate of copper oxide, which apparently dissolves only very slowly and in infinitesimal quantities in water spread on the surface of leaves by rains, mist, or dew."

It is generally known what has happened since. Following the communication, several large proprietors of Médoc did not hesitate to treat large areas of vineyards in accordance with the procedure I had worked out and recommended. The results were the most remarkable in the degree that my recommendations were faithfully followed in these treatments, and nowhere were they so complete as on the estate of M. Johnston of Dauzac and Beaucaillon, where the treatment had been directed by M. David who, for two years, had been experimenting on a small scale. The area treated by M. Johnston alone was about 25 hectares.

I should add another fact, still unpublished, in order that my statement may be complete. Having been consulted in the early part of August by one of my Italian correspondents M. Giuseppi Rebora, of Novi (Liguria), I described to him the treatment in question. As early as mid-August he applied it to 10 hectares of vines. Some neighbors followed his example. Among the persons to whom he described this method of treatment was M. le marquis G. Pinelli Gentile, of the Chateau Tagliolo, who treated 80 hectares of vines. Everywhere, the result was completely satisfactory.

I have given an account of these facts, save the last one, in my communication of October 5 before the Institute, which was published in the issue of October 8 of this journal. In two more recent notes, in collaboration with M. Gayon,[5] I pointed out the harmlessness to the wine from the treated vines and I fully explained the action of the treatment.

In the meantime, M. Perrey, who, on the 29th of September, evidenced the beneficial effects of sulphated props on mildew, tried a copper sulphate solution in Bourgoyne. He communicated to the Academy, for its October 5 meeting, detailed instructions concerning the technical part of the treatment, the same day on which I published the results of the treatments made at my suggestion at Dauzac. A solution of 5 per cent was applied by means of a hand sprayer. The total area of the 4 sections treated, although a little less than 1½ hectares, is entirely sufficient to judge the true worth of the procedure. It undoubtedly will be necessary to compare carefully from all points of view the one that I have proposed in order to determine the relative values of the one and the other.

[5] See numbers for 12th and 19th of November.

Finally, on the second of November, M. Muntz, professor at the Institute of Agronomy, wrote to the Academy concerning the results of experiments conducted by the same method in the Southeast, that is, by spraying a 10 per cent solution of copper sulphate. In that case, also, the treatment was followed by good results.

However, some other facts prior to those of which I have just spoken and of a different nature need to be added in order to corroborate or, rather, complete them.

In the past year, 1885, M. Lafitte, to whom the observations made in Bourgoyne the preceding year concerning the efficacy of sulphated props were known, began at Agen some careful experiments to clear up this fact, still contested by many persons in the Southwest. On the 22 of September he wrote to the Minister of Agriculture in order to lay before him certain remarkable results that had been obtained from these experiments.[6] They show, in fact, as clearly as can be desired, the salutary effect of the sulphated props and ties on mildew.

Others in Burgundy made some observations and experiments in 1885 that confirmed all of these facts. For example, MM. Magien,[7] Professor of Agriculture of Côte d'Or, Montoy, Vice President of the Committee of Agriculture of Beaune, and several others, the names of whom one may read in the report of M. Montoy.[8] One of the most interesting facts proved by the Burgundians is the efficacy of the sulphating of straw ties, of which they make use in this country to fasten the vine to the prop, or a small handful of sulphated straw, attached with the branches to the upper part of the props.[9]

One finds in the bulletin of which I am speaking an account of an experiment made by M. Bouchard, President of the Chamber of Commerce, which confirms and extends the conclusions of MM. Perrey and Muntz concerning the action of copper sulphate dissolved in water. A 3 per cent solution applied by means of the Riley sprayer was followed by very good results.

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[6] This relationship is reported in the October 1 number of this journal.

[7] See *Journal d'Agriculture*, No. for October 31, p. 712.

[8] *Bulletin du Comité d'Agriculture de l'arrondissement de Beaune*, October, 1885, p. 4 to 10.

[9] Ibid.

But the most important fact contained in the publication of which I speak is the corroboration of the efficacy of a dust with a base of copper sulphate and lime made by Louis Podechard, a winegrower of the hamlet of Gigny.

This dust is made essentially like the mixture I suggested. It is possible that it may prove to be a valuable addition or even a perfection of the latter. It certainly will be worth while to compare it carefully either with this or with the spray treatment proposed by M. Perrey, for nothing is proved by saying that a treatment that gives good results in Burgundy will suffice for the Southwest, and *vice versa*.

It remains for me to point out a recent contribution by M. B. Chauzit, Professor of Agriculture at Gard, who, by various kinds of experiments with copper sulphate, said he obtained only unsatisfactory results.

This hasty review would, I believe, suffice to make appreciable the part played by each in the great victory just obtained against mildew, if I had not purposely passed sketchily over the important facts in order not to prolong my account. I shall revert to them in closing.

On the first of last October, in inviting the public to assure itself of the beneficial results of the treatment at Dauzac I stated in the Journal of Agriculture and Horticulture of la Gironde, that I had first conceived, experimented with and recommended the practice of this treatment. When I say this *treatment*, I mean a treatment by copper. I would like to prove before putting my pen aside that this proposition is in no wise exaggerated and is, even today, a statement of fact.

It will be found that I first conceived the idea of a treatment by copper in a letter that M. David wrote me in December, 1884, in which he recalled the observation I had made in October, 1882, concerning the beneficial effects of the Médoc mixture of copper sulphate.

"You were right in attributing 2 years ago," he said to me, "to the spraying of leaves practiced along the highways to prevent pilfering, their good state of preservation and resistance to mildew." I hold the original of this letter at the disposal of any who may desire to be convinced of the truth of my assertion. It has been transmitted to the Society of Agriculture of la Gironde and is now in the hands of the National Society of Agriculture.

But another proof, still more decisive than the one of which I have just spoken, since it rests upon a well-known fact, is stated in a simple sentence that I cited at the beginning of this article and published, as has been seen, since the 15th of March 1883. I presented in it the opinion founded on recent observations that the sulphate of copper acted more effectively against mildew than all the others employed up to that time. Now, it seems to me, and I do not believe I am mistaken in this, that this simple observation possesses all the merit desirable in such a matter when it is shown that such observation was followed by several careful experiments, which would have been decisive in 1883 or 1884 had not chance prevented the results from being fully appreciated.

The proof that these experiments had been performed this same year, 1883, is found in another publication, which, like the preceding one, was presented before the commission named by the Society of Agriculture of la Gironde and has since been placed at the disposal of the National Society of Agriculture. There is an authentic copy of the passages in the original, with various notes, in the record of the work at the Chateau Dauzac during the occupancy of M. E. David, manager for M. Johnston, who reported on the experiments made at Dauzac, at my suggestion or inspiration, during the three preceding years. The document, which has not less than seven large pages, contains, with their respective dates, the account of the most important tests concerning the action of copper sulphate and sulphate of iron, whether as a dust or as a mixture with lime, on the dosage of the different elements in the mixture, on the other methods of treatment, etc. The first of these experiments dates from the middle of August, (the 18th, as near as I can recall, for I cite from memory) 1883. In 1884 these were conducted in the same manner. But it so happened that, in 1883 as in 1884, Dauzac was nearly free from mildew, a fact that prevented M. David and me from obtaining from these tests all the information we had a right to expect. Therefore, it was not until 1885, that is, three years from the inception of the research, that success crowned our efforts. There is no doubt that had mildew raged in Dauzac in 1883 our tests would have been sufficiently numerous and methodical to serve as a basis for entirely effective treatment. The incalculable

losses caused by the epidemic in these last two years would thus have been spared the wine growers.

I shall without doubt be granted the right to state that I first conceived and experimented with the copper sulphate treatment. It seems to me impossible that anyone would object to my adding the fact that I first recommended the use of the treatment. For this, as we have seen, is the treatment that has given remarkable results at Dauzac as recorded by Prillieux in his excellent report.[10] This treatment, I state, is found described down to the smallest detail in my communication to the Society of Agriculture of la Gironde on the 1st of April 1885. At this time, that is to say, at the moment when I was formulating the prescriptions for it, prescriptions that have received the sanction of the most striking success in France as well as in Italy, no one had anything other than more or less vague and incomplete data on the method of applying copper sulphate as a treatment for the epidemic. I claim the honor of having conceived the treatment with copper, that of having first experimented and, likewise, of having first proposed the practice. May I be permitted to add—for these are for us learned men our titles and our dearest souvenirs—that first, in 1878, simultaneously with M. Planchon, I observed the presence of mildew in France. Since then I have constantly been on guard. My work gives evidence of it.[11]

MILLARDET

November 21.

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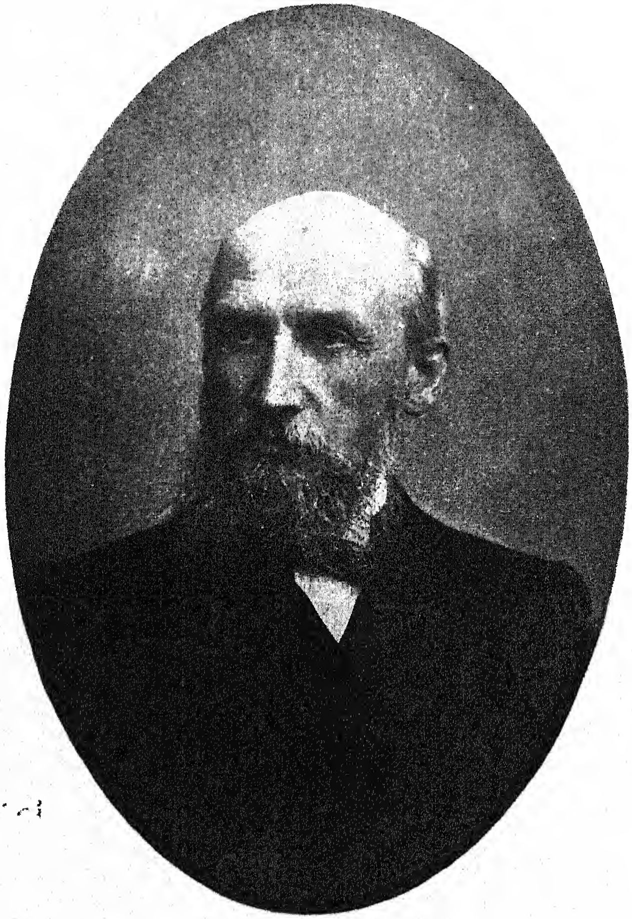
[10] I would have made many reservations as to the manner of presenting the facts in this report had not M. Prillieux, himself, declared that he alone intended to state the facts.

[11] I seize this occasion to reply to the objection made by M. Prillieux in the number of the journal preceding the last one (p. 723). It is true that the works cited by M. Prillieux preceded my memoir published in the *Zeitschrift*, etc. But, in my article: *Mildew in the Southwest in 1882*, printed in the August 21st 1882 number of the *Journal d'Agriculture pratique*, I gave the most circumstantial description of the *Rot of Jacques*, and, curiously enough, some of the same Jacques plants that served M. Prillieux for his notes of September 18 and October 2 of the same year. I therefore, reserve priority as I have stated.—M. Prillieux would not have raised this objection had my citation been more complete and precise. Of two notes that I was able to cite, I chose the most complete, the one that presents some figures but postdates the other by six months. I express my consequent regrets to my learned colleague of the Agronomic Institute. A.M.



Dr M. Woronin.
born at St. Petersburg
21 July / 2 August 1838.

photog. 1900.

Phytopathological Classics

NUMBER 4

PLASMODIOPHORA BRASSICAE

THE CAUSE OF CABBAGE HERNIA

By
MICHAEL STEPHANOVITCH WORONIN
1878

A Translation
from the German by
CHARLES CHUPP

With a Biographical Sketch
by the Translator

Published by the
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THE chief source of inspiration in the preparation of this publication has been Professor H. H. Whetzel. His original suggestion that the translation of Woronin's classical paper on club root be undertaken and his kindly prodding has from time to time revived our flagging spirit at critical moments.

To Dr. Wm. H. Weston of Harvard University we are indebted for the loan of Woronin's original article in Russian, published privately in St. Petersburg in 1877 and of which the well known German publication is an exact translation.

We wish to acknowledge our indebtedness to Miss Vera G. Turin, student at Cornell University, for her exceptionally well done translation from the Russian of the obituary of Woronin by Famintzin, which has served as the chief source of data in our preparation of the biographical sketch.

C. C.

MICHAEL STEPANOVITCH WORONIN

Michael Woronin was fortunate in a number of ways, which no doubt enhanced his success in science. He was well educated by a wealthy father, he had enough money of his own, so that he did not have to waste time in striving for a living, and had no family on which to lavish time and effort. Therefore, he was free to follow his investigations with undivided attention. He even had a laboratory in his bed room so that he did not need to waste time in going to or from his work.

But all the favorable surroundings imaginable can not make a successful man unless he has the ability to take advantage of every opportunity. Even though Woronin's brothers were given the same training as he, they never became renowned research men. Probably Michael, even when very young, had that infinite patience and deep sense of correct interpretation that later caused his work to be done so carefully that its scientific significance will endure as long as does the science of Phytopathology.

Michael Woronin (Voronin, in the Russian) was born in St. Petersburg July 21, 1838. His father spared no expense in giving him and his brothers the best training possible. His tutor was a German, who must have left a deep impression on the youth, who, through his entire life, cherished a well-known sympathy for Germany, its language, literature, and customs. At the age of sixteen Woronin entered the University of St. Petersburg where he studied natural history and especially geology. When he graduated in 1858 his thesis on granite and diorite rocks was so outstanding that he was presented with a silver medal.

After the commencement, he immediately went to Germany in company with his classmate Famintzin, to whom we are indebted for almost all the biographical material on Woronin now extant. It is true that Nawaschin, also a Russian and a coworker, wrote a very flowery eulogy of his friend, but contributed very little to our knowledge of the actual facts of his life.

Woronin spent two years abroad studying botany, at first in Heidelberg under Holle, and later in Freiburg in the then unassuming laboratory of Anton de Bary. De Bary's presentation must have been very impressive to young Woronin who became his teacher's close friend and correspondent and who modelled his methods of research after those of this great master. Not only that but he finally gave up all other botanical subjects in favor of mycology, especially the development and life histories of fungi, a subject then being made so interesting by the investigations in de Bary's laboratory at Freiburg. Notwithstanding his later inclinations, his first work under de Bary was the preparation of a paper on the stem anatomy of Calycanthus.

During the summer of 1860 he worked on sea algae in the Gulf of Genoa and later wrote an article on *Acetabularia* and *Espera.* This paper served as a thesis for his Master's Degree in Botany which he received from the University of St. Petersburg in 1861. This was his first and last endeavor in the procuring of a degree.

During the next dozen years, he worked so assiduously and published such clearly stated and beautifully illustrated pieces of research, that he began to be well-known in all botanical circles, and organizations felt proud to do him honor. The new University of Odessa in 1874 gave him an honorary degree of Doctor of Botany. He was invited to membership in several Naturalists' societies, the Russian Olericultural Society, German Botanical Society, the Linnean Society of London, and he was made an honorary faculty member of the Royal Universities at Dorpat and Kharkow.

Woronin apparently was jealous of his time for study, and took part in very few outside activities. He was a private docent in the University of St. Petersburg from 1869 to 1870 where he lectured on mycology. From 1873 to 1875 he lectured on mycology and on the morphology of the cell at a medical school for women. His one hobby seemed to be the secretaryship of the botanical section of the Natural Science Society of St. Petersburg. He served faithfully in this position for thirty years. It gave him an opportunity to review all the new botanical literature and keep in touch with workers throughout the entire world.

A man in so prominent a position naturally was called upon to solve a number of immediate problems in phytopathology. His kindliness of spirit never permitted him to refuse such help as he could give, even though it took him away from his favorite group of fungi, the Sclerotiniæ. To this category of work may be referred his paper on "intoxicating bread," in the South-Ussurian Region. Bread made out of certain rye flour caused, headache, dizziness, and vomiting. Woronin with his usual thoroughness found that fifteen different fungi were associated with the grains of rye and that four of these were the most dangerous ones in producing the ill effects when taken within the human body. For some reason or other, he was at one time requested to study the flora of the large intestines, a report of which he made to his society later. Another problem was the sunflower rust, which was driving peasants out of the sunflower industry. Woronin not only worked out the life history of the fungus, but in his final publication suggested a number of practical control measures.

Probably his greatest contribution to the pressing plant-disease problems of his time was undertaken not through a direct request by the people, but because he wished to aid the market gardeners in the vicinity of St. Petersburg. This was his classic investigation dealing with the club-root of cabbage. This work was done so carefully and recorded so accurately that, although many papers have been written since on the subject, little has been added to that originally observed by him.

No doubt a bedroom laboratory in St. Petersburg would pall upon even the most monkish type of investigator if the monotony were not broken by outside activities. At least Woronin found it desirable to make fairly frequent trips to Germany where he applied himself, either alone or in cooperation with some other worker, to various types of investigations. But, apparently, by far the most pleasant outings were his summer trips to his villa in Finland. Here he entertained his friends by taking them to the cool bogs nearby where representatives of many genera of the heath family grew luxuriantly and where Sclerotiniae were present in great abundance. Studies on the life history of these forms then took the place of recreation in his life. He was never too busy to discuss any phase that might be concerned

with these cup fungi. Naturally, the investigation finally widened to include the monilioid forms causing brown rot of cultivated fruit.

The subjects that have been mentioned do not nearly complete the record of Woronin's research activities. Famintzin concludes his biography with a citation of sixty-four papers published by Woronin. Nawaschin also included the same list in his tribute to the author.

A man of Woronin's wealth must often have been solicited for financial help. At least it is known that he willingly aided in several educational causes. Among these were donations to the University of St. Petersburg for the building of the Botanical Institute and for the establishment of a Biological Station at Bologoé. He made it possible to publish a review of the scientific activities of the Botanical Section of the Natural Science Society of St. Petersburg for a period of twenty-five years from the time of its foundation. Furthermore, he usually paid for the insertion of his own well-drawn illustrations (frequently colored) and in this way was of direct service to the publications in which his articles appeared.

According to his two biographers, Woronin was ever kindly disposed toward the people he met. He was studiously careful not to hurt the feelings of any one. He was liberal in time and money. He was painfully modest and abhorred argumentation. All these attributes endeared him not only to his fellow workers but also to the average man about town, so that he was mourned by a large circle of friends when he died February 20, 1903.

—Charles Chupp

Famintzin, A. S. Obituary of M. S. Woronin. Report of the St. Petersburg Imperial Society of Naturalists **34**:1-13. 1903. (Same article reprinted in Travaux du Musée Botanique de l'Académie Impériale des Sciences de St. Petersburg **2**:1-13. 1905). This article has been translated into English by Vera G. Turin. Copies of this are available in the Library of the Plant Pathology Department of Cornell University.

Nawaschin, S. Michael Woronin. Berichte der Deutsche Botanische Gesellschaft (Generalversammlungs Heft). **21**:(35)-(47). 1903.

PLASMODIOPHORA BRASSICAE THE CAUSE OF CABBAGE HERNIA*

by M. Woronin

During recent years the "hernia" of cabbage plants has become very widespread and at the present time is causing serious losses to the vegetable gardeners in many Russian communities, especially in the environs of St. Petersburg. The cause of this disease and the methods for its control have remained unknown until now; consequently, the Royal Russian Gardening Society (in St. Petersburg) in 1872 posted a prize for a scientific investigation on the subject. The latest date on which such a work could be handed in was set as November 1, 1873. When at the termination of this period nothing had been offered, the date was advanced two years, that is, until November 1, 1875; and when the second announcement remained without results the date again was extended two years, or until November 1, 1877.

For my part, I undertook the investigation of the cabbage hernia quite accidentally and wholly independent of the announcement by the committee. I began the work in 1873 and did not complete it until the autumn of last year (1876); thus the discussion that I present here is the result of three years' work. I succeeded in discovering that the actual cause of the disease is a new organism, which I have named *Plasmodiophora brassicae*. I investigated its life history as thoroughly as possible and by means of cultural experiments proved to myself that this organism is the only cause of cabbage hernia. After I was sure of the pathogen, it was not difficult to turn to control measures, which, though not completely eliminating the disease, might appreciably decrease its development.

* (Translator's Note:—Although this translation has been made from the German edition published in the *Jahrbücher für wissenschaftliche Botanik* Vol. XI, a careful comparison with the original Russian edition published privately in 1877 under the same title shows the German to be an exact translation. M. Woronin's first publication on club root was: Die Wurzelgeschwulst der Kohlpflanzen, Botanische Zeitung **33**:337-339. 1875. This is a summary of his talk before the Botanical Section of the St. Petersburg Naturalist's Society, March 5, 1874).

I made a preliminary report of my investigations to the Botanical Division of the St. Petersburg Natural Science Society, February, 1874[1]; later to the fifth gathering of the Russian Natural Science Society in Warsaw; and to the Botanical section of the St. Petersburg Natural Science Society, November 18, 1876.

I obtained the materials for my investigations, that is, cabbage plants affected with hernia, in St. Petersburg and its immediate vicinity (among others, also in the village of Vira in the Zarskoie-Selo district) as well as in the region of Wiborg (in Finland).

I take the opportunity here to express my deepest appreciation to those who came to my aid with their cooperation, either in providing needed materials or by drawing my attention to several statements in literature.

I extend especial thanks to Messrs. P. Wolkenstein, E. Regel, E. Gratscheff, N. Kniper, E. Junge, C. Gernet, C. Maksimowitsch, A. Batalin, E. Ender, A. Hackmann, and Mrs. P. Tarnowsky.

I

The disease of cabbage, known generally by the Russian vegetable gardeners under the name of crucifer hernia (Kapoustnaja kila), is portrayed by characteristic swellings of various shapes and sizes on the roots of affected plants. The disease rarely is confined to a very few swellings on a limited number of roots, and in such cases the plant suffers no injury, or at least none that is noticeable. Much oftener the swellings appear in great numbers and in such masses that all the roots are covered. The crowded roots of such diseased cabbage plants, being entirely disfigured, have a wholly different appearance from those of the healthy plant. Such an extreme development of the root hernia produces on the cabbage plant a very pronounced injury, which is made evident on the part of the plant above ground either by the total absence of a head or of one that is much retarded in its development.

It is possible to find cabbage plants with well-developed heads and yet have roots that are partly or wholly covered

[1] This report has been made available also in the German language. (Bot. Zeitung, 1875, p. 337).

with the hernia swellings. Such cases occur if the infection takes place when the cabbage plant is well-grown rather than when it is very young. This shows that the plant is susceptible in all stages of development, and, in fact, hernia can be found on cabbage from early spring, while it is still a seedling in the hot bed, until late autumn, after the heads have been cut from the stalks.

The form of these heterogeneous crooked swellings of the root—these hernia excrescences—usually is so variable and irregular that it is very difficult, yes, even impossible, to give a suitable and correct description of them. I therefore refer to figures 1-5 (Plate 29) and figures 6-11 (Plate 30), in which the cabbage hernia is illustrated in natural size and in its most variable stages of development.

The size of these most abnormal and, for the most part, formless root protuberances is quite variable, as may be judged from the illustrations. The most remarkable excrescences I had the opportunity to examine were the size of a healthy man's fist. Incidentally, it may be mentioned that the very largest hernia swellings are found mostly on the taproot; the swellings that occur on the secondary roots are, as a rule, much smaller.

The color of the hernia protuberances is the same as that of the healthy roots; namely a greasy-gray or pale yellow. In a cross section the excrescence is snow white and of a fairly firm texture, being as easily cut with a sharp knife as is a potato tuber or any turnip. This, however, is true of the hernia swellings only in the beginning; later they wrinkle, shrivel, and decay, take on a dark color, and finally become a rotten, decayed, almost liquid-like, dark brown mass, which usually emits a very strong foul odor. So far as I was able to observe, the hernia swellings in dry soil exist for a fairly long time without any visible change. On the other hand, in wet ground and especially after a heavy rain of several days they rot extremely rapidly. In the process of decay the parenchyma tissue of the root falls completely apart, while the vascular bundle retains for some time the form of fiber bundles. (Compare Plate 29, Fig. 2; Plate 30, Fig. 6). In most cases the decay begins at the lower part of the taproot and on the secondary roots that are deepest in the ground. While these lowest excrescences are decaying gradually, new

roots grow on the upper part and in turn also become diseased and covered with hernia swellings, after which the process is again repeated. Finally, new roots are put out from the base of the cabbage stem and even from the leaf scars on these stems. In vegetable gardens one can find such cabbage plants, which are fastened to the soil only by these upper roots, part of which are above ground, while all the remaining roots are not only diseased, but already appear half-decayed.

The hernia disease attacks all kinds of crucifers, the common cabbage (white, as well as red), cauliflower, borecole, savoy, kohlrabi, all cabbage and turnip varieties (*Brassica Rapa* and *Br. napus*), rape, etc., and not only all species of the genus Brassica, but also several other plants of the crucifer family; for instance, hernia is found on *Iberis umbellata* (Plate 30, Fig. 9) and on stock.

The hernia seems to be a general and widespread evil; at least, it is well known not only in Europe but also in America. It apparently is present in the whole of Europe. I, myself, have found it and investigated the disease in Russia (in the environs of St. Petersburg and Warschau) and in Finland (near Wiborg). Aside from this I have a long list of accounts, both private and in literature regarding the presence of this disease in England, Scotland, Belgium, France, Germany, and Spain. In England hernia is known as Clubbing, Club-root, Ambury, Anbury or Hanbury, or also as Finger-and-toes.[2] It is well known by the last name also in Scotland and America. The Belgians name it "Vingerziekte" or "maladie digitoire." In Germany the hernia goes by the name of "Kelch or Kropf des Kohles." It is found in many localities in Germany; for instance, in the environs of Hamburg, along the Rhine, in the Harz Mountains, in Würzburg, and in Prussia (in Gumbinnen).[3] The Spaniard, Ruiz Diaz de Isla, one of the oldest syphilographers, says that in his country syphilitic swellings occur

[2] Compare Gardener's Assistant, p. 245 and 361; Charles Mintosch: The Book of the Garden, 1:111; Johnson: The Cottage Gardener's Dictionary p. 28.

[3] I obtained these data from *Hamburger Garten- und Blumenzeitung* by Ed. Otto (vol. 9) Hamburg 1853, 1:433, 477, 514, and 535; and from *Zeitschrift des Landwirthschaftl. Vereins für Rheinpreussen,* (No. 5, vol. of 1853). Regarding the appearance of hernia in Prussia (*in Gumbinnen*) compare R. Caspary: Eine Wruke (*Brassica napus* L.) mit Laubsprossen auf knolligern Wurzelausschlag in the *Schriften d. Physik.—Oekon. Gesellschaft zu Köningsberg* vol. of 1873, p. 109.

on cabbage plants. From his description, the swellings are undoubtedly nothing more than cabbage hernia.[4]

Although hernia is so generally distributed, an investigation of the disease has been wholly neglected, even to the present day. In recent years it has developed most rapidly in Russia,—a phenomenon very easy to explain. In no other country is cabbage grown so intensively as in Russia. According to an estimate made by Mr. E. Gratscheff (one of the most prominent vegetable gardeners of St. Petersburg) the annual income derived from cabbage growing in the St. Petersburg district alone amounts to the impressive sum of 300,000 rubles (about $153,000). The Russian truck gardeners suffered especially in the late 60's and early 70's so that, for example, in 1869, according to the statement of E. Gratscheff, half of the cabbage crop about St. Petersburg was destroyed by hernia. Such a severe attack by the disease naturally caused the vegetable growers a very considerable loss, so that finally they were forced to pay adequate attention to the appearance of the trouble. This gave rise to the Vegetable Gardener's Association of St. Petersburg, an organization for the inauguration of a scientific investigation of cabbage hernia.

If now we turn to literature, we do not find one work in which the hernia is subjected to a strictly scientific investigation. Because of this, it is conceivable why the true cause of the outbreak has remained wholly unexplained until the present day. Most of the land owners and gardeners who have studied the matter closely have come to the conclusion that the disease is produced by a whole series of insects, among the most important of which are the following forms: *Anthomyia brassicae, Anthomyia trimaculata, Curculio pleurostigma, Curculio contractus, Centorhynchus sulcicollis, Centorhynchus assimilis.*[5] This, however, is an error, the insects

[4] I give here the exact words of Ruiz de Isla, which I quote from Dr. E. Lancereaux's *Traité historique et pratique de la Syphilis* second edition, Paris, 1873 p. 591. Ruiz de Isla says as follows: "At Baiza in my own country, I have noticed cabbages attacked by syphilis. This disease is communicated to them by stagnant water in which the linen of syphilitic patients has been washed and which has been used afterward to water the plants. The swellings of these resemble the pustules of the disease to such a degree that the children cut them with shears and paste them on their faces to imitate the disease. Besides, also other vegetables, as well as a great number of animals, suffer from the malady."

[5] Compare Rob. Thompson: *Gardener's Assistant,* 1:245 and 361; G. Johnson: *The Cottage Gardener's Dictionary* (1857), p. 28; C. Mintosch: The *Book of the Garden,* 2:111; Boisduval: *Essai sur l'Entomologie horticole,* p. 145; Taschenberg's *Ento-*

being merely secondary. Usually no insects are found in fresh protuberances of hernia as long as they remain firm, but do appear in the swellings that have begun to decay—a thing that already has been observed in England.[6] The excrescences in their decay and disintegration play the same rôle for insects as do any other decomposing organic substances, such as meat, fruit, mushrooms, and the like. Here the insects lay their eggs and the larvae develop in the rotting mass. Similarly, the insects may be observed to follow the hernia and in no case are the cause of it. As early as 1872 Mr. A. Wehitschkoff (farmer and landlord of Gschatsch) remarked quite correctly that cabbage hernia was not caused by insects.[7]

So far as the botanists are concerned, not one of them has conducted a careful investigation of the disease. J. Kühn (*Die Krankheiten der Kulturgewächse*, 1858, pp. 252 and 253) differentiated two types of swellings on cabbage roots. According to him, the one was produced by *Anthomyia brassicae* and the other, J. Kühn likened to the swellings that occur on the trunks and branches of conifers (for example, *Pinus picea*) as well as those that appear on beets, and in whose development, as he says, no insects take part. The excrescences on cabbage plants were mentioned only briefly by P. Sorauer (*Handbuch der Pflanzenkrankheiten*, 1874, p. 167) who attributes them to *Ocyptera brassicaria* F.

The only and at the same time perfectly correct illustrations of hernia (on rape) are given by R. Caspary.[8] Although he made a microscopical-anatomical study of the protuberances, he was not fortunate enough to discover the real cause

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mologie in the Russian translation by Baillon (1871).—In the *Berichte des Schulactes der Petrowsky-Rasoumowsky 'schen Landwirtschaft- und Forst-Akademie in Moskau* for the year 1873 it says on page 33: "The cabbage and turnip plants were heavily infested by three species of cabbage flies (Anthomyia). The roots of such plants were seriously injured by these insects and were covered with swellings which disappear in decay." In one of the *Sitzungen der Kaiserl. Russischen-Gartenbaugesellschaft in Petersburg* (for Jan. 15, 1872) Mr. Iversen declared that he had investigated the cabbage hernia collected in Mr. Gratscheff's gardens and hoped that by spring (1873) he would be able to identify the insects. He was of the opinion then that not only a Coleopter but a Dipter was involved.

6 Compare the English literature cited in the previous foot note.

7 Compare "*Die Berichte der Kaiserl. Gartenbaugesellschaft zu St. Petersburg,*" 1872, p. 440 (in Russian).

8 R. Caspary: "Eine Wruke (*Brassica Napus* L.) mit Laubsprossen auf knolligerm Wurzelausschlag" in *Schriften der Physik-Oekon. Gesellschaft zur Königsberg*, 1873, p. 109; Plate XIV, Fig. 1-3—"Hereditary Deformity in *Brasscia napus*" in the *Gardener's Chronicle*, 3:148, Fig. 25, Feb. 1877.

of their origin. He found, he says, "No sign of fungous hyphae or insect stings or any other exterior injury that presumably could produce such nodules."

R. Caspary paid less attention to the excrescences themselves than he did to the leaf buds that arise from them. I often observed such leaf buds on the root swellings of hernia not only on rape but also on common cabbage (Plate 30, Fig. 6), and especially did they develop luxuriantly on kohlrabi. The appearance of leaf buds on the root excrescences of hernia is of itself a most interesting phenomenon, which, so far as I know, never occurs on healthy roots and appears only on diseased cabbage roots that are deformed by hernia.

The real cause of hernia remained unknown up to the present day, and I was fortunate enough to unravel the mystery while investigating the swellings on diseased roots. I found a new organism, which I have named *Plasmodiophora brassicae*. It dawned upon me gradually from my study of its life history that it must be regarded as the only cause of cabbage hernia.

R. Caspary published a short note in the *Gardener's Chronicle* (February of this year) in which he announced the results he obtained when he sowed the seed of diseased rape. He considered the appearance of the heterogeneous nodular swellings on the rape and the development of leaf buds on the abnormal roots as a natural, constant phenomenon. In his opinion, they were reproduced inheritably through the seed. Undoubtedly this assertion is false.*

The results announced by R. Caspary can be explained very simply as follows: he planted the seedlings which he grew from the seed of diseased rape in soil in which without doubt spores of Plasmodiophora were present; and in my opinion, the cause of the affected seedling was then due to these spores. So far as the leaf buds on the roots are concerned, they can develop into new individuals, as R. Caspary's own experimentation showed, and can even play the rôle of seedlings. I should give no one the advice, however,

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* Translator's Note: Woronin, M. Nachträgliche Notiz zur Frage der Kohlpflanzen-hernie, *Botanische Zeitung* 38:54-57. 1880. This was a correction of his previous opinion that all swellings on roots probably were caused by an organism like Plasmodiophora, and to notify the public that he was wrong in believing that Caspary's "Monstrositus" on *Brassica napus* was a form of hernia.

to employ them in this manner, for all these leaf buds are already affected with the disease; in other words, they already contain the Plasmodiophora (compare Fig. 45 of Plate 33).

II

Before I begin the description of Plasmodiophora, I should like to discuss briefly two not unimportant related topics.

1. In the spring young cabbage plants, while they are yet in the hot bed, often may die; but the cause of this is in no way related to the development of Plasmodiophora. The outer parenchymatous tissue of the hypocotyl of the young seedling, while still in the cotyledonous stage or at most bearing only 2-3 pairs of stem leaves, begins to decay at the joint. It usually takes place where the stem joins the root near the surface of the ground. The delicate stem of the young plant becomes limp, bends over until it lies on the ground, and then generally rots away. A microscopic examination has shown that this destruction of the young seedlings is caused by a Chytridium that penetrates the roots and the hypocotyl stem and there achieves its full development. I am proposing the name *Chyt. brassicae* for this new Chytridium. The structure and complete development of *Chyt. brassicae* is exactly like that of the other Chytridium species. The following species, however, are most closely related to it: *Chytridium* (*Olpidium*) *endogenum* Al. Br., *Chyt. apiculatum* Al. Br., *Chyt. saprolegniae*, Al. Br., (*Olpidiopsis saprolegniae* Cornu) and *Olpidiopsis incrassata* Cornu.[1] Because of the great similarity to the above named forms and aided by the illustrations in figures 12-18 (Pl. 31), the description can be given very briefly.

Each globose zoosporangium of *Chyt. brassicae*, as shown in the illustrations, possesses a slender neck, the length of which may vary greatly. The zoosporangia that cling in the epidermal cells of the hypocotyl joints or in the cells of the root epiblem have very short necks, while those in the deeper tissue, that is, the cortex, always are seen to have a much longer neck. It is not unusual to find such necks that had to

[1] Compare Al. Braun: Ueber Chytridium 1856, p. 61.—M. Cornu: Monographie des Saprolégniées. *Ann. des sc. nat. V. séries,* 15:145-146, pl. 3 and 4. 1872—N. Pringsheim: *Jahrb. für Wissenschaftl. Botanik II*. p. 219. Pl. XXIV.

break their way through 3 or 4 or even 5 layers of cells before they reached the surface of the host plant. This zoosporangial tube opens on the surface of the host to discharge the zoospores. I found many zoosporangia, however, that opened within the host and liberated the zoospores into the cells of the parenchyma, which sustained the fruit body.—The zoospores of *Chytridium brassicae* (Pl. 31, Fig. 15), are composed of a naked, globose, fairly uniform mass of protoplasm, which, as is true of all the swarmspores in the genus Chytridium, possesses a single cilium, and within the mass contains a small vacuole as well as a hyaline oil-drop-like nucleus mostly of very slight dimensions.

Aside from the zoosporangia, *Chytridium brassicae* possesses also other reproductive organs, namely the resting spores (resting cells, which are nonmotile). I found these in the epiblem cells of the roots (Plate 31, Figs. 16, 17, 18). They are colorless or faintly yellowish, have a proportionately thick membrane, and are more or less star-shaped which gives them a certain resemblance to the cysts of Amoebae or Infusoria. The protoplasmic content of these bodies is hyaline, finely granular, and usually provided with a larger or smaller oil-droplet. I was unable to follow either the development or the further fate of these bodies. According to analogy with similar resting spores found in other Chytridiaceae and lately carefully investigated by Cornu and Nowakowsky,[2] one may safely assume that the resting spores of *Chytridium brassicae*, like zygospores, originate through the fusion of two protoplasmic bodies and, after a definite period of rest, are transformed into zoosporangia.

2. I wish now to make several brief remarks regarding the structure of young and perfectly healthy cabbage roots. These show a rather interesting peculiarity in their structure, which, so far as I know, has been considered by no one. Its endodermis or protective sheath[3] is surrounded by a pa-

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[2] M. Cornu: Monographie des Saprolégniées. *Ann. des sc. nat. V. séries*, 15:120, 137, etc. 1872.—L. Nowakowski: Beiträge zur Kenntniss der Chytridiaceen in die Beiträgen zur Biologie der Pflanzen by F. Cohn. vol. II, part 1, p. 73 and part 2, p. 201.

[3] Schutzscheide (R. Caspary), Gefässbundelscheide, Vaginalschicht, Plerom- oder Strangscheide (J. Sachs), Endodermis (Oudemans, de Bary).—According to de Bary's proposal the generally accepted term "Schutzscheide" should be replaced from now on by Oudeman's prior designation, "Endodermis." (Compare A. de Bary: Vergleichende Anatomie der Vegetations-organe der Phanerogamen und Farne. in *Handbuch der Physiolog. Botanik*, 3:129. 1877).

renchymatous tissue whose structure reminds one of the so-called secondary or outer protective sheath, present in the roots of several conifers, according to the investigations of Ph. van Tieghem,[4] J. Reinke,[5] E. Strasburger,[6] J. Klein,[7] and others. This peculiarity which until now has been considered as a characteristic feature of only a few conifers[8] is present also in cabbage plants, as is shown by my present investigations.

In the young roots of cabbage plants, each of the most deeply embedded cells of the endodermis immediately adjoining the periblem sheath has a peculiar continuous membranous thickening on its radial and cross walls, as is true of the conifers. This extends into the lumen of the cell in the form of a padded band, and the marvel of it is that the united paddings of all the cells of the periblem sheath always correspond to each other on their inner sides (compare Pl. 31, Fig. 20-23). In a tangential section the thickened rings of each of these periblem cells appear in the form of four-cornered cavities which are packed into the cell and serve to a certain extent as a support. Taken together, these thickenings form a lattice work, that completely surrounds the central cylinder or plerome of the root. It is not unusual, as may be seen in figure 20, to find these peculiar thickenings not only in one but in two contiguous rows of the inner periblem cells, a phenomenon that previously has been observed in several conifers; for example, in *Thuja*.—The characteristic part of the structure of these cells lies in the fact that a very thin and delicate net-like enlargement is present on their inner tangential walls. These nets arise from the above described thickened bands by growing, so to speak, out of these enlargements and extending quite regularly along the inner tangential walls. (Pl. 31, Fig. 22 and 23). So far as the outer tangential walls of these inner periblem cells are concerned, they have at most only a very slight thickening, which does not produce a fully formed net, but

[4] Ph. van Tieghem: Recherches sur la symétrie de structure des plants vasculaires. *Ann. des sc. nat. V. série* 23:5.

[5] J. Reinke: Morphologische Abhandlungen, Leipzig 1873.

[6] E. Strasburger: Die Coniferen und Gnetaceen, Jena 1872, p. 340.

[7] J. Klein: Zur Anatomie junger Coniferen-Wurzeln, *Flora* 1872, p. 81. Nachtrag, p. 103. Weitere Beiträge zur Anatomie junger Coniferen-Wurzeln. *Flora* 1872, p. 385.

[8] According to the investigations of van Tieghem, E. Strasburger, J. Reinke, and J. Klein, this characteristic is found in the Taxineae and the Cupressineae, but is not present in the Abietineae.

merely the slightest suggestion of one. These enlargements have here the form of very slender simple or branched veins, which also arise from the thickenings already mentioned so repeatedly (Fig. 22). In the young root of the cabbage plant, as in roots of other dicotyledons, the outer parenchyma (the so-called periblem) dies when the plerome begins to enlarge, and gradually is totally sloughed off as far in as the endodermal sheath (Pl. 32, Fig. 24); the inner row of periblem cells that has the above described lattice-like thickening also dies and is sloughed off with the remainder of the periblem sheath.

III

I now take up the description of Plasmodiophora.

If two cross sections are made, the one through a young, perfectly healthy cabbage root (Pl. 32, Fig. 25) and the other through a root of the same age, but already affected somewhat with hernia (Pl. 32, Fig. 26) and the two cross sections are compared, it may be observed that the difference between the two lies in the mere fact that in the diseased root (Fig. 26) several cortical cells are filled with an opaque, hyaline, finely granular, protoplasmic substance, and that the cells immediately surrounding these individuals are slightly enlarged in comparison with the adjoining cells. If a series of cross sections is made of a cabbage root that is slightly more affected (Pl. 32, Fig. 27) and these are examined carefully under a microscope, one soon comes to the conclusion that the gradual enlargement of these hernia swellings is not due altogether to the extreme growth of the individual cells of the cortical element, but also to their great increase in number through cell division. In the largest swellings of the cabbage hernia not only the cells of the cortex but also the vascular element appears abnormally developed or altered. As is shown in the illustration (Pl. 32, Fig. 28), the fibrovascular bundles not infrequently take on an irregularly variable distortion and thereby are shifted from their original normal position. The appearance of such misshapen and distorted fibrovasuclar bundles in the hernia excrescences has already been described by J. Kühn[1] and R. Caspary.[2] In this connection it may be remarked that the

[1] J. Kühn, l.c. p. 253.
[2] R. Caspary, l.c. p. 109.

irregularly distorted vascular ducts in the hernia swellings contain merely air as is true of the ducts in the normal, healthy roots. But so far as the greater part of the cortical cells in the cabbage root protuberances are concerned, some contain, as has been mentioned above, a colorless, finely granular, dense protoplasm, while others are filled completely with minute, globose, colorless bodies. The finely granular protoplasm is the plasmodium and the small globose bodies, the spores of the organism that I have named *Plasmodiophora brassicae*. This organism, which in its structure and development belongs to the Protista (according to the definition of E. Häckel) and is to be considered as one of the very simplest Myxomycetes, penetrates the young, healthy cabbage root, develops there, lives as an obligate parasite, and thereby produces in the tissue the variation and distortion that has already been described.

The plasmodium of Plasmodiophora is constructed like any other plasmodium that might be mentioned. It is composed of a hyaline, transparent, somewhat viscous, amorphous slime substance, in which are embedded colorless, minute granules and oil-droplets. The number and size of the vacuoles in the plasmodium are quite variable, often appearing in whole masses. In the beginning it is almost impossible to differentiate between the plasmodium and the cell contents; it is difficult to say with full assurance which part of the cell has been engulfed by the plasmodium and in which part nothing but the cell protoplasm is still present. Not infrequently the plasmodium appears to be the sole content of the cell that is being studied.[3] At first, the plasmodium is so unusually pale that it is scarcely distinguishable; but, as it develops further and enlarges the host cell, it gradually becomes opaque and takes up more of the entire lumen of the cell (Pl. 32, Fig. 29-34; Pl. 33, Fig. 35, 36). If the plasmodium of Plasmodiophora is to be examined the sections must not be cut too thin, or the water in which the mount is made will rush into the cells containing the plasmodia and will destroy them almost in a moment. It is thus

[3] It is no new idea that the plasmodium can live in the protoplasm of another living organism and develop at its expense. Especially good examples of this are furnished by the Chytridiaceae (e.g. Olpidiopsis, Rozella, Woronina), which have been described by Cornu (l.c. p. 119, 132, 133, 149, 170, 172) as internal parasites of the Saprolegniaceae.

possible to see that the plasmodium is in the form of protoplasmic globules of various sizes, which flow out of the injured cells in the section (Pl. 33, Fig. 37). The globules of the plasmodium, when lying in the water, soon disintegrate by bursting and dissolving. The same phenomenon occurs here as is true in other similar cases, e.g., when the plasmodium of *Aethalium septicum* is dissected or when a vesicle of Vaucheria is injured (compare J. Sachs: *Lehrbuch der Botanik*, IV. Auflage, page 42).

The normal, uninjured plasmodium of Plasmodiophora possesses, in common with all other plasmodia, the power of motion, a movement that manifests itself with the greatest deliberateness. One can make sure of this only by watching the same cell fixedly for a long while. Although it is very difficult to prove absolutely, after further corroborative phenomena one may safely accept the fact that the plasmodium not only moves about in the lumen of one cell but that it oozes from one cell into another. It is very evident that this passing from one cell to another occurs in no other way than through the sieve plates or groups of pits that are present in the walls of all parenchyma cells of the cabbage root (compare Pl. 33, Fig. 40, a, b, and c.[4]) If starch grains are found in the protoplasm of the parenchyma cells of a diseased cabbage root, they are there because they were drawn in together with some of the cell protoplasm by the plasmodium of Plasmodiophora. In other words, it is evident that the plasmodium feeds on these (compare Pl. 33, Figs. 39 and 41). In most cases the plasmodium of Plasmodiophora does not at first entirely fill the lumen of the host cell, but occupies only some definite part. It may lie at one end of the cell (Pl. 32, Fig. 32; Pl. 33, Fig. 39) or form a peripheral girdle, or lie as a cross beam in the middle part (Pl. 33, Figs. 35, 39) or finally, which is much more seldom, it balls up into clumps in the middle of the cell and from this central mass delicate protoplasmic strands radiate out in all directions toward the periphery of the cell (Pl. 33, Fig. 36). After the development of the plasmodium is somewhat more advanced,

[4] According to the latest investigations of C. Cornu, the protoplasm possesses the ability to pass from one cell to another through a living membrane that has neither pores nor pits. (Compare M. Cornu: Sur le cheminement du plasma autravers des membranes vivantes non perforées. *Comptes Rendus* 1877, Premier Semestre, No. 3, p. 133).

it usually takes possession of the whole lumen of the host cell, or at least the greater part of it, and appears at this time to have its whole mass spread out very uniformly. Soon thereafter the whole plasmodium breaks up into spores.

The process of spore formation is similar to that of zoospore formation (compare Pl. 33, Figs. 41-44). The very first step is the arrangement of small globular vacuoles equally distant from each other throughout the whole mass of the plasmodium, dividing it into a fine protoplasmic network or lattice (Pl. 33, Fig. 41). After this a gradual change takes place in the formation. The vacuoles of this network begin to disappear, and simultaneously, the granular substance of the plasmodium lying between the vacuoles collects into small uniform spheroid aggregations. In place of the vacuoles there now appears in the plasmodium an equally large number of small, globose, increasingly well-defined bodies (Pl. 33, Fig. 42, 43). These bodies are the future spores of the Plasmodiophora. They usually fill completely the entire lumen of the host cell and are stuck together as with putty by the colorless water-transparent remnant of the plasmodial slime substance. In this manner the cluster-shaped aggregations of spores usually retain in their contour the size and form of the supporting parenchyma cells.

Special emphasis must be placed here on the fact that no other envelope than the mere cellulose membrane of the containing parenchyma cells surrounds the spores.[5] In all the Myxomycetes (with the single possible exception of Ceratium), even in so simple a form as e.g. Dictyostelium, the spores are surrounded by a mutual covering—a peridial membrane, the structure of which in several forms appears rather complicated. Plasmodiophora has nothing of the kind. The wall of the parenchyma cell in the cabbage plant in which the small, hyaline, globose spores are enclosed replaces here the true sporangial wall. In Plasmodiophora no trace is to be found of a capillitium, which is present in so many Myxomycetes.

In all the parenchyma cells that contain plasmodia of Plasmodiophora, spore formation gradually begins. At

[5] In my previous contribution regarding Plasmodiophora (*Botan. Zeitung* 1875, p. 337) it was stated that in each parenchyma cell a hyaline, transparent, very delicate enveloping membrane was present between the spores and the cell wall. I must retract this statement. It was an error into which I fell easily because my investigations had only just begun at that time.

about the same time the whole group of root swellings begins to decay, a process that is much hastened if the affected cabbage plants are growing in wet soil.[6] If hernia swellings far advanced in decay are examined under the microscope, it is observed that the soft rotting mass is made up almost wholly of free-lying parenchyma cells most of which are completely filled with the aggregations of mature spores (Pl. 33, Fig. 46; Pl. 34, Fig. 47). During the decay the swellings undergo a sort of maceration and disintegration into their several elements. By the still further decomposition due to rot, the walls that confine the spores in the parenchyma cells are reabsorbed and the smeary liquid into which the hernia mass has dissolved is made up almost completely of Plasmodiophora spores, which in part are fully separated and free, while the remainder still lie heaped together (Pl. 34, Fig. 48, 49).

The number of spores is extremely large. The spores are unusually small, their largest measurement not exceeding 1.6μ in diameter. They are perfectly globose and only very rarely are they biscuit-shape (Pl. 34, Fig. 49 and 50). There are double spores, which apparently arise from two spores, that do not separate when they are molded from the plasmodium. The structure of the spore can be observed only by a magnification of 700-900 diameters. They are hyaline with a delicate and perfectly smooth wall, and have a colorless, finely granular protoplasmic content (Pl. 34, Fig. 50, 51).

If the hernia swellings remain long in damp soil they disintegrate completely, thereby liberating the spores in the soil so that the young roots of perfectly healthy plants may become infected by them. The further development of the spores lying free in the soil consists in the swarming of a myxamoeba from each one (compare Pl. 34, Fig. 52). After the myxamoeba has oozed out of the spore and is lying free in the water it possesses a somewhat elongated, spindle-shape body, provided with a rather long, whip-like cilium at its beaked, sharply pointed, anterior end. In the naked protoplasmic body of the myxamoeba there are always present a slowly pulsating vacuole and several small granules, one of these granules usually has larger dimensions than the others.

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[6] It has already been stated that the hernia swellings do not decay in dry soil, but under such conditions remain whole, without any apparent change, for a longer or shorter period.

The motility of these myxamoebae is quite characteristic. First, the cilium always is pointed forwards; secondly, the motion is not due exclusively to the cilium but also to the very lively weaving from one side to another of the supple beak; and thirdly, aside from the free movement of the Plasmodiophora myxamoeba as described above, it exhibits still another quite characteristic movement, which resembles creeping or stepping. A delicate thread-like projection stretches out from the lower or posterior end of the protoplasmic body with which the myxamoeba fastens itself to any suitable object lying under the water.

The myxamoeba now contracts this projection and quickly extends another, fixing it to the same object or to some other one lying near by, and so forth; thus without exaggeration it can be said to take real steps (Pl. 34, Fig. 52). The myxamoeba of Plasmodiophora shows also amoeboid movement, as is common to all other myxamoebae (Pl. 34, Fig. 53); but this movement generally occurs later, that is, after the myxamoeba is a few days old.

The myxamoebae of Plasmodiophora force themselves from the soil into the young healthy cabbage roots. Although I was not fortunate enough actually to observe penetration under the microscope, I am convinced of the fact that the myxamoebae enter the cabbage plant through the root hairs and the epidermal cells. I arrived at these (in my opinion) perfectly safe conclusions by the following cultural investigations. I filled flower pots with good, rich garden soil to which I added a given quantity of well-rotted hernia swellings, and sowed fresh seed of various cabbage varieties in these pots. I permitted the young plants produced from these seeds to continue their growth in the pots and watered them daily with water in which fully rotted hernia excrescences had been finely crumbled—water containing a very large quantity of Plasmodiophora spores. Hernia swellings, although as a whole rather small, yet perfectly normal and well developed, appeared on almost every root of the young plants grown in this manner (Pl. 29, Fig. 4, 5; Pl. 30, Fig. 10, 11). On the other hand, no trace of hernia swellings appeared on the roots of cabbage plants grown from the same seed but in soil that was not inoculated with rotting hernia swellings and that were watered with distilled water containing no spores of Plasmodiophora. These roots remained

perfectly healthy throughout the entire experiment. Having the same end in view, still another series of cultures was run through at the same time in the following manner. Young plants were placed in shallow vessels, such as large watch glasses or in porcelain evaporating dishes or on a glass slide, without any soil, but having their roots in water to which had been added a definite quantity of mature spores of Plasmodiophora. I was unable to grow cabbage plants in such water cultures long enough for their roots to show hernia excrescences, but a microscopic examination of the root hairs and epidermal cells of such plants proved very instructive. First, I found these root hairs irregularly swollen in a quite variable manner, even entirely deformed; and secondly, I found in many of them as well as in the epidermal cells an unusually delicate, fully transparent plasmodium, which had exactly the same appearance as does the plasmodium of Plasmodiophora (Pl. 34, Fig. 54 and 55). The experiments here outlined together with my investigations are, so it seems to me, wholly sufficient to assume with absolute correctness that the myxamoebae of Plasmodiophora derived from the soil penetrate the young cabbage roots through the root hairs and the epidermal cells. One may further assume from other experiments of a similar nature that the myxamoebae can force their way into the root and produce infection, not only when it is young, but at a much later stage; that is, the myxamoebae are able to penetrate the roots after these have sloughed off their primary cortical tissue, and are found in the second stage of root development. After they have entered the cells of the root cortex, the myxamoebae of Plasmodiophora mingle with the cell contents, consume it, wander from one cell to another, and thereby produce a violent irritation in all the tissues. The cells suffer an intense hypertrophy and, in the cells of such diseased, much enlarged tissues, there develops from the small microscopic myxamoebae new plasmodia that later are transformed into small globose spores.

From what has been said here of the structure and development of Plasmodiophora, the conclusion may be drawn that it is an extremely simple organism. It is composed merely of a protoplasmic mass—a plasmodium, which, during its whole lifetime, is never enclosed in a membrane

of its own and finally disintegrates into an immense number of small spores, each of which produces a myxamoeba. Each of these myxamoebae enters the tissue of the cabbage root and forms within it a new individual—a new plasmodium. One question that I was unable to answer was whether the plasmodium of Plasmodiophora in the host cell arises from a single myxamoeba or from the fusion of several. This second possibility seems to me by far the more plausible.

Because of the simplicity of its whole structure, *Plasmodiophora brassicae* is a true Protist (according to the definition of Häckel) and therefore stands closest to the Myxomycetes. Cornu (l.c., p. 120) has already pointed out that the Myxomycetes are closely related to the Chytridiaceae; and through Plasmodiophora the affinity stands out still more plainly. Plasmodiophora, in common with the Myxomycetes, possesses a plasmodium that, after a certain time, breaks up into an unbelievably large number of small globular spores that later produce myxamoebae. Plasmodiophora, however, differs sharply from all the other myxomycetal forms in the total absence of a true sporangial membrane and because of its parasitism within another living organism. In every other way, but particularly its manner of living, *Plasmodiophora brassicae* resembles most closely the Chytridiaceae.

IV

Since I have shown conclusively in the foregoing chapter that cabbage hernia is caused by Plasmodiophora, there remains yet the following question to be answered: Is it possible to control this disease? If this is not wholly attainable, is there some means of appreciably lessening the development of Plasmodiophora and thereby reducing the injury to the cabbage plant?

In my opinion the absolute eradication of hernia on the cabbage plant is impossible. It is unthinkable that any substance should kill the plasmodium and spores of Plasmodiophora and at the same time preserve the protoplasm and tissue of the cabbage root in which the Plasmodiophora is parasitic. This substance, whatever it might be and in whatever manner it might be applied, would destroy the cabbage plant when it killed the Plasmodiophora.

The following methods are suggested for definitely checking the development of Plasmodiophora and thereby reducing the injury that the hernia causes on the cabbage plant:

1. The principle method of control and possibly the only real one against hernia is fire. Cabbage stumps usually are left in the field after harvest and by next spring most of them are rotted completely. Later, when the new seed bed is being prepared, the remainder of the cabbage stems and roots are mixed with the soil into which the seedlings are set. This, however, should not be permitted under any consideration. On the other hand, in the fall, or right after the heads are cut off, all the cabbage stems and roots in the vegetable garden should be gathered carefully and burned on the spot. In this manner an appreciable quantity of hernia swellings, in which are a great mass of Plasmodiophora spores, are destroyed in each vegetable garden. The ashes that remain after the stumps are burned can be utilized as fertilizer on other cabbage crops.

2. In the spring, when the plants are set from the seed bed into the field, the strictest and most careful sorting should, by all means, be practiced. Every cabbage plant that shows the slightest trace of hernia should be removed from the vegetable garden and destroyed in no other manner than with fire.

3. Aside from what has been said above, the spread of cabbage hernia can be much reduced if all of the vegetable gardeners use a rational crop rotation in the culture of cabbage. It has long been known and proved by much experience that this practice is of great importance to the grower. Regarding this, compare, for instance, R. Schröder: Russicher Gemüsegarten, Pflanzschule und Obstgarten, Petersburg, 1877, p. 3-4. (In Russian).

R. Schröder[1] gives the advice to the Russian gardeners that in the growing of cabbage they should practice rotation to such an extent that cabbage will not be planted on the same soil oftener than once every two years.

The remedies I have suggested for the control of cabbage hernia are:

1. The burning of the old useless cabbage stumps and their roots.

[1] R. Schröder is of the opinion that hernia on cabbage plants is caused by the cabbage fly (*Anthomyia*).

2. A strict sorting of cabbage seedlings before they are set into the field.

3. The introduction of a strict, rational crop rotation as a control measure is nothing new within itself, having been introduced among the English and Russian gardeners some time ago, but not enough care was taken in its practice. I am firmly convinced that if these measures are accepted and followed closely there need be no complaint regarding any unusual development and spread of the cabbage hernia.

Still other control measures have been suggested for cabbage hernia, but in my opinion they are not of great importance, and I doubt if their application could be of any real value to the cabbage plants. For instance, in England soot is added to the soil on which cabbage is to be grown and the cabbage plants, before they are set into the field, are dipped in water to which soot has been added. The English, as well as the Germans, remove the hernia swellings by cutting them off with a sharp knife, and find the operation unusually worthwhile, especially when the removing is done while the plant is still young.

The application of guano, saltpeter, salt, bone meal, wood ashes, or other phosphate to the soil can play the rôle only of any other good fertilizer but can not eradicate hernia from the cabbage field.

V

Excrescences that are similar in many ways to the hernia on cabbage are found on many other plants. The real cause for the development of most of these abnormal growths remains almost wholly unknown to us at the present time. The statements that we have regarding them are not at all satisfactory. For example, one may consider as incomplete my work[2] on *Alnus glutinosa* and *Lupinus mutabilis* and that of Eriksson[3] with the legume plants.

I found a parasitic fungus, which I named *Schinzia alni*, in the excrescences on the roots of black alder (*Alnus glutinosa*). J. Eriksson and I found in the root nodules of *Lupinus mutabilis* and of those on *Faba vulgaris* a peculiar

[2] M. Woronin. Ueber die bei der Schwarzerle (*Alnus glutinosa*) und der gewöhnlichen Garten-Lupine (*Lupinus mutabilis*) auftretenden Wurzelanschwellungen. *Mem. de l'Acad. Imp. des Sciences de St. Petersburg, VII—séries* **10**: No. 6, 13 pp. 1866.

[3] Jakob Eriksson. Studier öfver Leguminosernas Rotknölar. Lund. 1874.

parenchyma tissue, the cells of which were filled with small bodies, and that I probably wrongly took for bacteria or vibrio-like structures. Further research should give us a more intimate knowledge not only of the structure but also of the true cause of the appearance of such root nodules. Similar investigations would most undoubtedly reveal still other new parasites that might be similar to *Plasmodiophora brassicae* or possibly identical with it.

In conclusion, I wish to impart an opinion that possibly might be of value to the animal pathologists. I suspect that the appearance and development of many pathologically abnormal growths and swellings, present on animal organisms, may be explained in the following manner: Small myxamoebae in their various ways gain an entrance into the living organisms where they gradually develop into plasmodia and produce a strong irritation in the tissue of the host organs. This brings about in all the tissues of the organ a pathological change on which the form and size of the diseased growths or swellings are dependent. Further investigations will show whether my supposition can be verified.

EXPLANATION OF ILLUSTRATIONS*

Plate 29

Fig. 1.	Roots of a young cauliflower plant covered with hernia swellings. These plants were taken from the hot bed April 28 to May 10.
Figs. 2 & 3.	Hernia swellings on the common (white) cabbage.
Figs. 4 & 5.	Beginning of hernia swellings on roots of young cabbage plant. (All illustrations on this plate are natural size).

Plate 30

Fig. 6.	Hernia swellings on common (red) cabbage, (a) adventitious leaf buds; natural size.
Figs. 7 & 8.	Swellings on common turnip. Nat. size.
Fig. 9	Hernia root swelling on *Iberis umbellata*, natural size. I obtained this illustration from Mr. O. Wolkenstein.
Fig. 10.	Beginning of hernia on the roots of ordinary (white) cabbage. Nat. size.
Fig. 11.	Commencement of clubbing on the young roots of cabbage seedlings, grown in pots and artificially infected with spores of Plasmodiophora. (a) Nat. size; (b) observed under low magnification of a hand lens.

Plate 31

(Figs. 12, 19, and 20 are magnified 90 times; Fig. 13, 160 times; Figs. 15-18 and Figs. 21-23, 520 times).

Fig. 12-18.	*Chytridium Brassicae*, n. sp.
Figs. 12-14.	Zoosporangia of *Chytridium brassicae* in different stages of their development.
Fig. 15.	Zoospores of *Chyt. brassicae.*
Figs. 16-18.	Resting cells or resting spores of *Chytridium brassicae.*
Fig. 19.	Cross section through a very young, perfectly healthy root of a seedling cabbage plant.
Fig. 20.	Central portion of a cross section through a somewhat older root. In this section is very well shown the characteristic thickening that occurs in the cells of the inner periblem sheath (a S) per. = pericambium (= couche rhizogène of van Tieghem), S. = the endo-

* Translator's Note. These figures are reproduced from those in the original Russian publication. They are here reduced to approximately ⅔ the size of those in the original plates.

dermis (= couche protectrice of van Tieghem, (Schutzscheide of R. Caspary); r, the young side roots growing out from the pericambium.

Fig. 21. A small portion of the above section, much more enlarged, as, S, and per. have the same appearance as in Fig. 20.

Figs. 22a & 22b. Cells of the inner periblem tissue in the tangential longi-section, taken from the inner portion where they are immediately surrounded by endodermis.

Plate 32

(Fig. 27 is magnified 75 times, 24, 25, 26, 28, 90 times; and 29-34, 520 times).

Fig. 24. Cross section through a young healthy root in the stage when the inner parenchyma—the periblem—is already dying and has been sloughed off and the further development is continued in the plerome.

Fig. 25. Cross section through a perfectly healthy root, not affected at all by the disease.

Fig. 26. Cross section through a somewhat older root in which already appear signs of the hernia disease.

Fig. 27. Cross section of a cabbage root, already badly diseased.

Fig. 28. Part of a section through a cabbage root that is still more seriously attacked by the hernia. The bundles present are pushed from their former position and possess various abnormal spiral bands and irregular distortions.

Figs. 29-34. Parenchyma cells, taken from various hernia swellings and containing Plasmodiophora plasmodia. In Fig. 34 the plasmodium contains a few starch grains.

Plate 33

(Figs. 35-39, 40a, 41-43 are enlarged 520 times; Fig. 40b, 712 times; Fig. 45, 90 times; and Fig. 46, 160 times).

Figs. 35 & 36. Parenchyma cells of a diseased root, which contain the plasmodia. In the plasmodium of Fig. 35 some starch grains are found.

Fig. 37. Protoplasmic bodies dissolved in water that occurs when cells containing the plasmodium are cut through with a sharp knife or otherwise injured. In these protoplasmic bodies also lie some starch grains.

Figs. 38 & 39. Parenchyma cells from a clubbed root in which are contained a few further developed plasmodia. In Fig. 39 there are starch grains. Some of these are entirely surrounded by the plasmodium.

Fig. 40. Parenchyma cells of a diseased cabbage root, in the membrane of which are shown sieve-like groups of pits.

Figs. 41, 42 & 43. Plasmodia shown in the act of spore formation. In Fig. 41 are starch grains.

Fig. 44. A semidiagrammatic drawing representing the gradual process of formation in plasmodiophora.

Fig. 45. Cross section through the lamina of the leaf derived from a leaf bud on a clubbed root (Compare with Fig. 6 on Plate 30). As is to be seen here, these leaf buds also are injured somewhat by the disease. Plasmodiophora is already in them and appears in all their further development.

Fig. 46. Free-lying parenchyma cells, appearing as if macerated the most of which contain matured spores. The entire slimy mass consists of such cells and results from the disintegration of the roots having hernia.

Plate 34

(Figs. 47, 48, 49 are magnified 320 times; Fig. 50, 51, 712 times; Figs. 52, 53, 620 times; Fig. 54, 90 times; Fig. 55a, b, and c, 160 times; Fig. 55d, 520 times).

Fig. 47. One of the free-lying parenchyma cells, as in Fig. 46, in which are contained mature Plasmodiophora spores.

Fig. 48. A mass of mature Plasmodiophora spores. The membrane of the host cell is entirely absorbed and the spores, which have been held together in one mass, are about to separate from each other.

Fig. 49. Spores lying free. Two of them double, biscuit-shape.

Fig. 50. Double spores of Plasmodiophora, much enlarged.

Fig. 51. Mature Plasmodiophora spores.

Fig. 52. Spores of Plasmodiophora at the time of myxamoeba formation and the myxamoebae swarming free in the water.

Fig. 53. Myxamoebae of the Plasmodiophora about 6 days after their development from the spores. A pulsating vacuole appears in these myxamoebae.

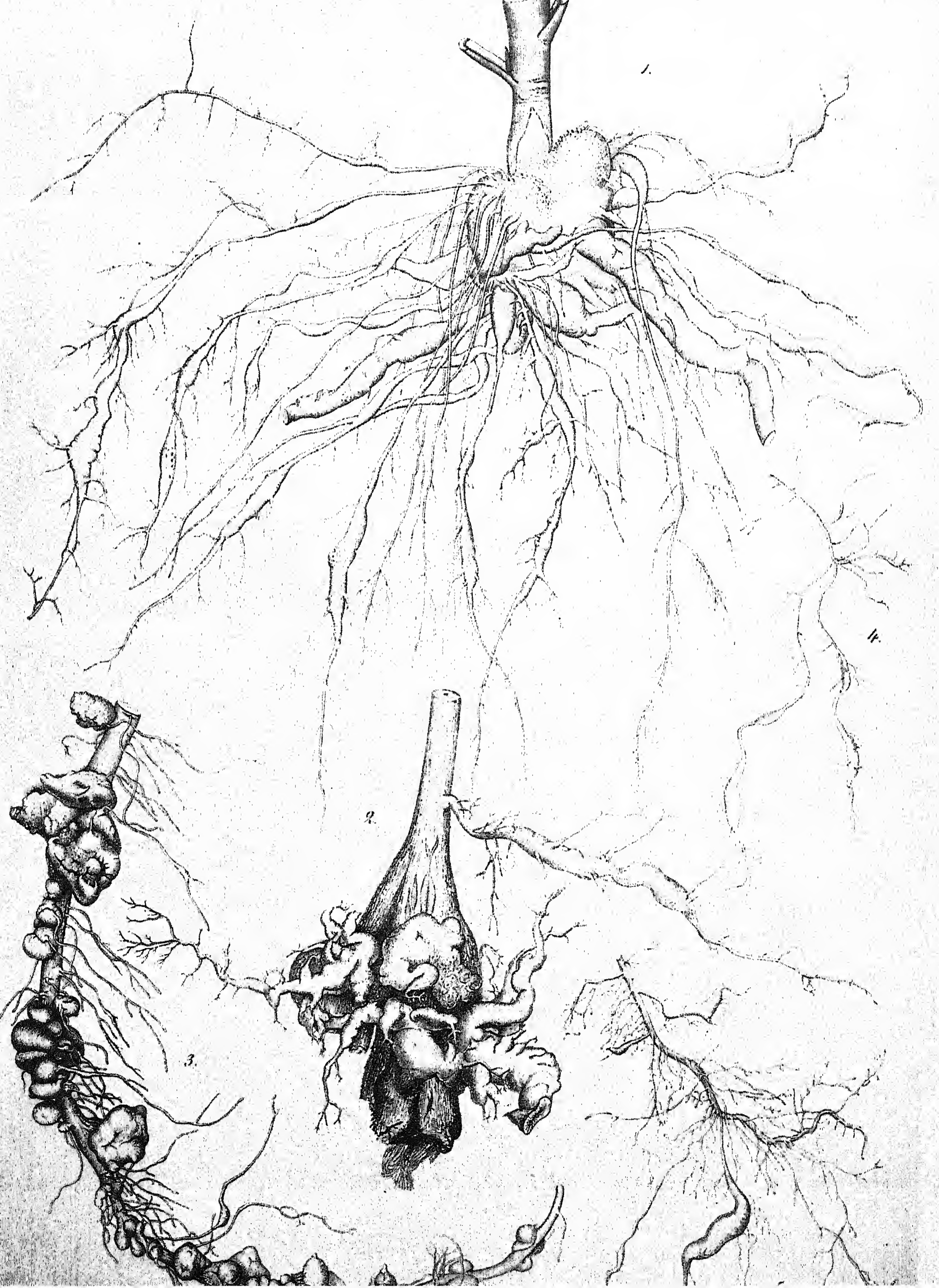
Fig. 54. Cross section through a young root of a seedling cabbage plant that has been infected artificially with Plasmodiophora spores.

Fig. 55. Root hairs of a cabbage plant, which was grown from seed, on a watch glass in water containing Plasmodiophora spores. In these root hairs are found plasmodia, which are very delicate and almost transparent. 55d is only a portion of 55c with much greater magnification.

PLATES

PLATE 29

Труды С. Петерб. Общ. Естествоиспыт. Томъ VIII. Таб. I.



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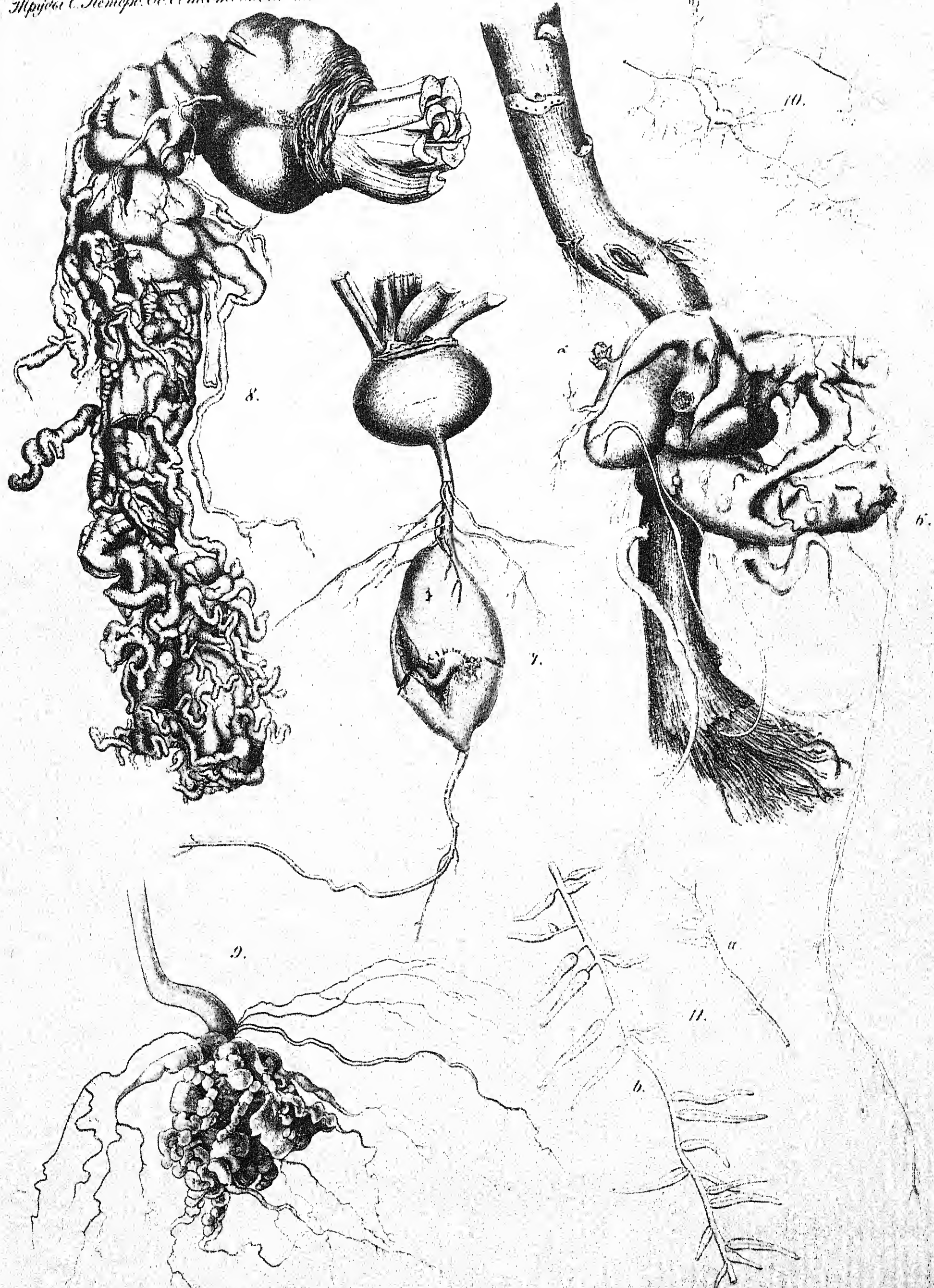
Таб. II.



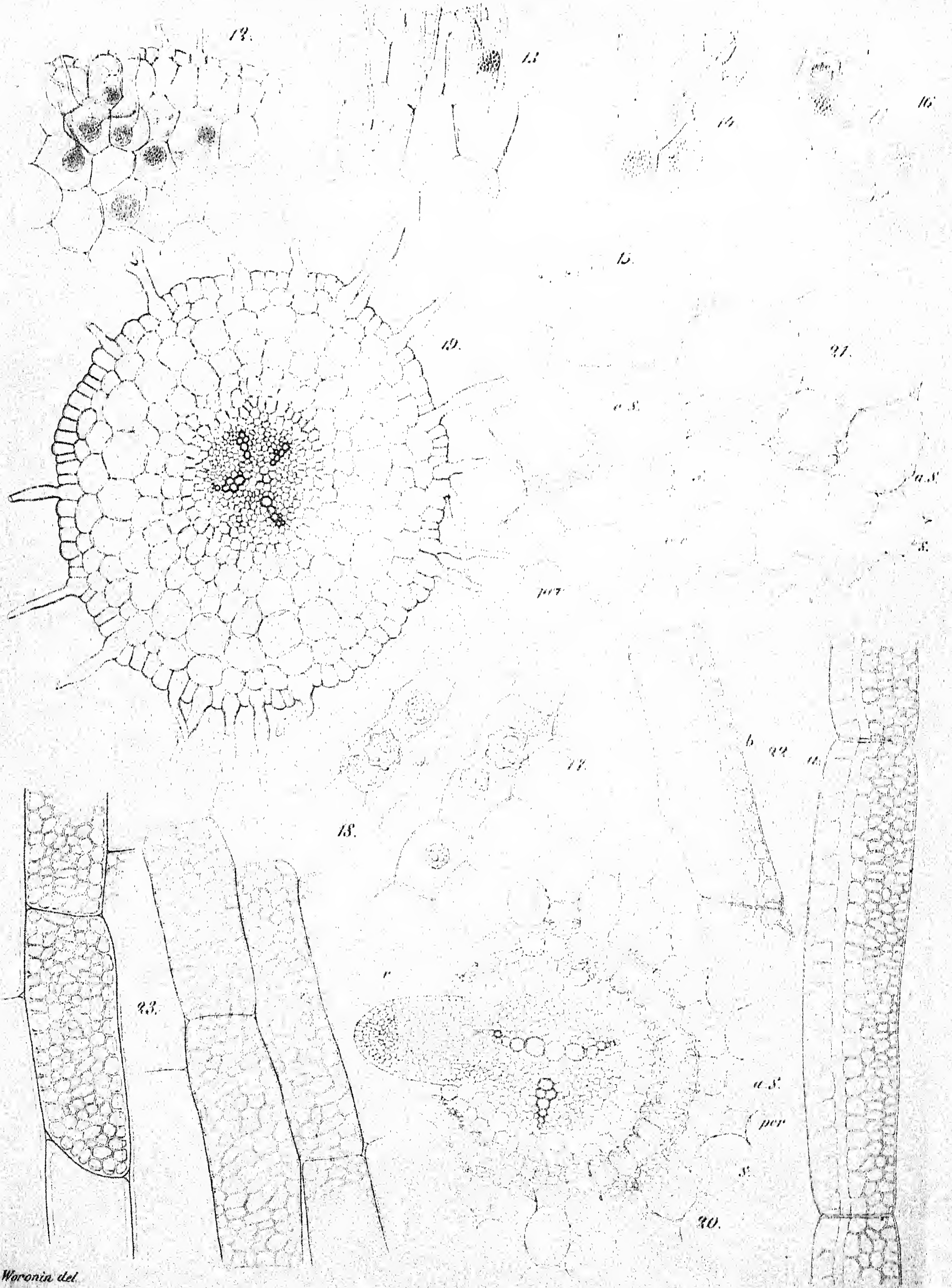
M. Woronin del.

Lith. von Laue

PLATE 31

Труды С.Петерб. Об. Естествоиспыт. Томъ VIII

Таб. III



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Труды С. Петерб. Об. Естествоиспыт. Томъ VIII. Таб. IV.

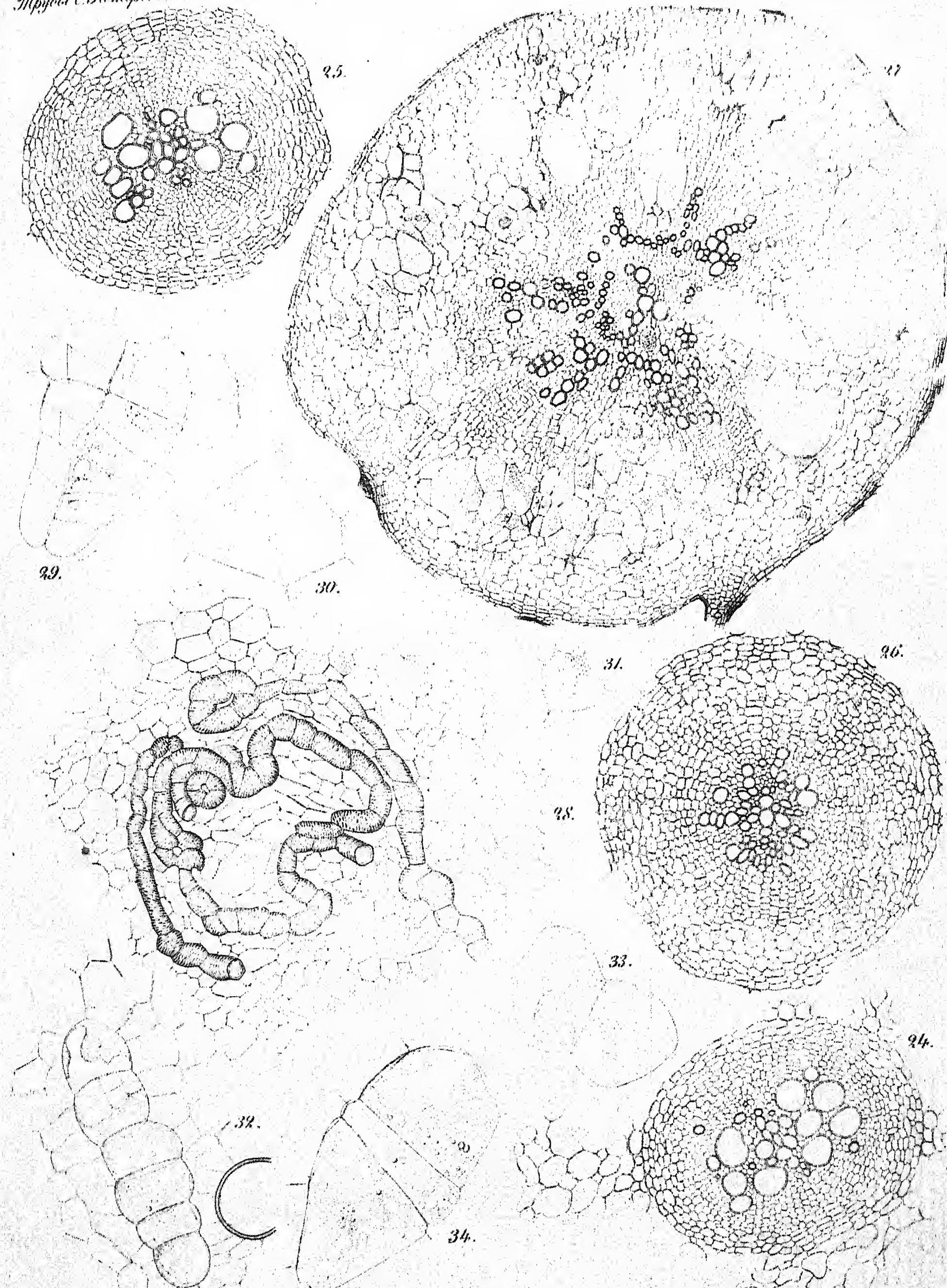

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PLATE 33

Труды С. Петерб. Общ. Естествоиспыт. Томъ VIII.

Таб. V.

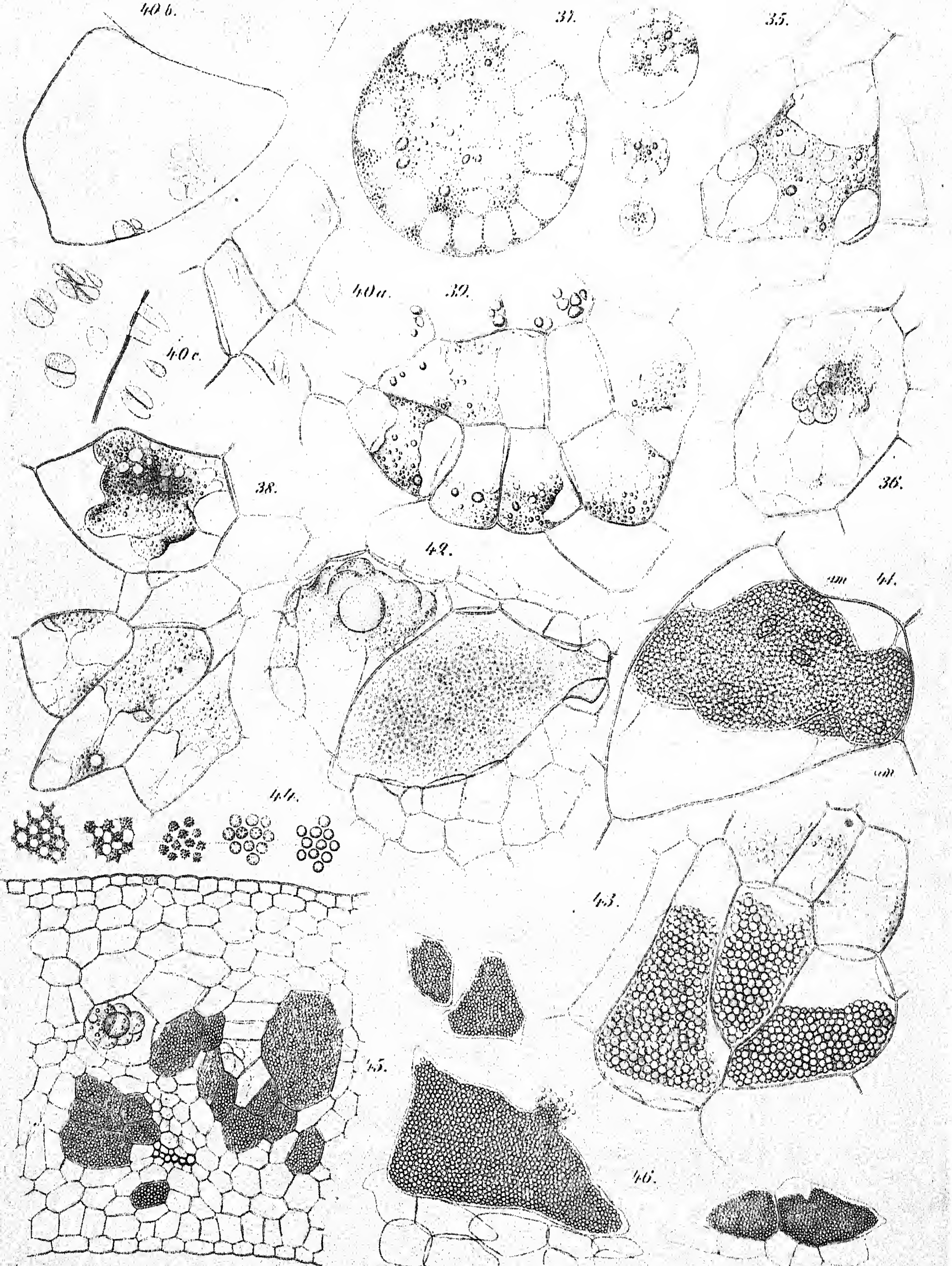

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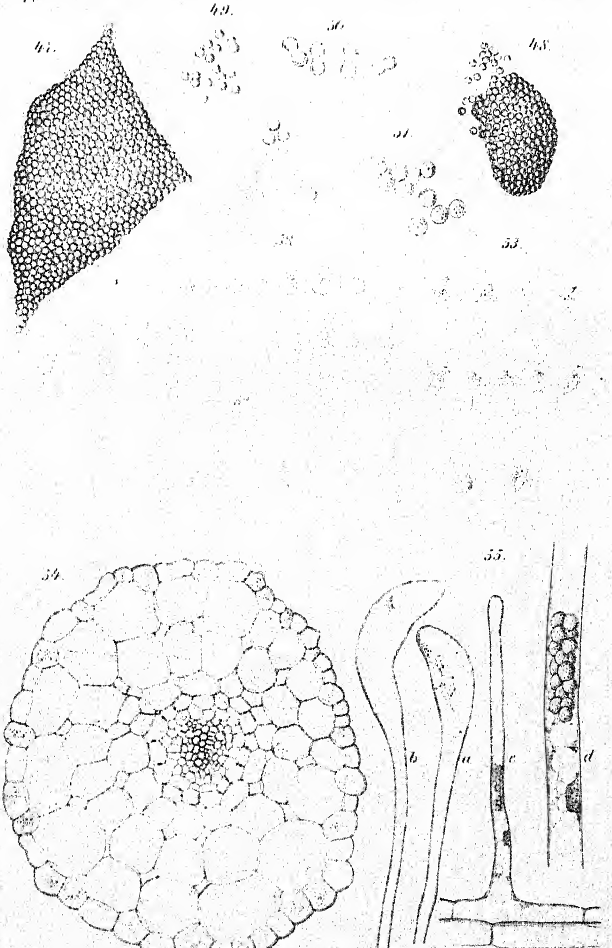
PLATE 34

Труды С. Петерб. Общ. Естествоиспыт. Томъ VIII. Таб. VI.



M. Woronin del. Lith. von Laue

DISSERTATION
SUR LA CAUSE
QUI CORROMPT ET NOIRCIT
LES GRAINS DE BLED
DANS LES ÈPIS;
ET SUR LES MOYENS
DE PREVENIR CES ACCIDENS.

Qui a remporté le Prix au Jugement de l'Académie Royale des Belles-Lettres, Sciences & Arts de Bordeaux.

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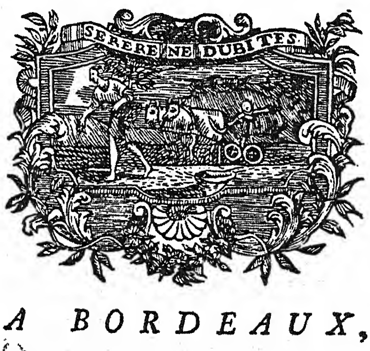

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Phytopathological Classics

NUMBER 5

DISSERTATION ON THE CAUSE OF THE CORRUPTION AND SMUTTING

of the

KERNELS OF WHEAT IN THE HEAD

and on the means of preventing these untoward circumstances

Awarded the Prize by the Royal Academy
of
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of Bordeaux

by

M. Tillet, of Bordeaux, Director of the
Mint of Troye

TRANSLATED FROM THE FRENCH
by
HARRY BAKER HUMPHREY

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INTRODUCTION

M. Mathieu du Tillet, one time Director of the Mint in Troyes, France, and author of numerous important papers on bunt of wheat and on the control of the Angoumois moth, was born in Bordeaux, France, in 1730, or thereabouts, where he applied himself during his spare time to the improvement of agriculture. In addition to his contributions pertaining to improved farm practice, he published important papers on the economic milling of cereals and especially that of wheat.

The amount of available published information concerning this scholarly and industrious man, who labored so effectively for the advancement of experimental science, is seemingly very meager. Nothing apparently is known concerning his early education, and we are left with the impression that, although a man of parts, he was modest withal, even to the point of self effacement. His published work would indicate that he must have been a man of unusual iniative and energy.

Single-handed and alone he applied himself assiduously to the development of the experimental method of solving problems in agronomy. Modern science owes to Tillet, and men of his scholarly instinct, a debt of gratitude that can never be paid. He lived at a time when men were much given to the more "gentlemanly pursuit of speculation" as to the cause of any natural phenomenon. The inductive and objective approach to problems in natural science had yet to be accepted as the truly scientific method of discerning and interpreting the facts.

Obviously, Tillet, at the outset of his career as an experimentalist, was not an agronomist, nor was he a botanist; he was, however, a born investigator. All he needed was a problem plus the opportunity and facilities for its solution. With unswerving zeal and fidelity to purpose he applied himself to the task of first putting under the hammer of relentless experimental test and analysis every

theory that up to his time had been heralded as a possible explanation of the cause of the then already well-known bunt of wheat. This he did with gentlemanly respect to authors and adherents alike of such hypotheses and deductions as rested on other foundation than that of proved fact.

He had first to clear his own thought of the trammels of deductive reasoning and speculation about natural phenomena; indeed, his masterpiece, *Dissertation sur la cause qui corrompt et noircit les grains de bled dans les épis* may be regarded, in a sense, as a history of the development of a scientific mind. It is probably true that when he first began thinking about the subject of his *dissertation* it was his intention to subject his materials to the acid test of well-checked experimentation. And yet, it is equally true that he was not wholly divorced from the temptation to draw conclusions from mere observation and *à priori* reasoning. Witness, for example, his treatment of the subject of ergot in cereals and grasses. After due consideration of the explanations advanced by Languis, Fagon, and others none of which was based on experimentation, he falls into the same pit and concludes that ergot is caused primarily by the sting of an insect. It is, however, conceivable that, had he devoted as much thought to the study of this disease as he did to that of bunt, he would have resorted to the experimental method with the same assiduity and patience he applied to his investigation of bunt.

Tillet did not succeed in discovering the fact, later disclosed by Prévost, that the primary cause of bunt was, in truth, a parasitic fungus; but he did very definitely prove that the disease resulted through contamination of the seed with the bunt dust. At no time did he suspect the true origin and function of this dust, but concluded that it was the active agent in transmitting a virus to the wheat plant, a poisonous principle that was self-perpetuating in its power to reproduce more of the same sort of virus-transmitting dust.

It is truly remarkable that here was a man who, without the certain advantage of training in agronomy and botany, had the genius to carry to successful issue research of such far-reaching importance. Without instruction and alone, he planned comprehensive experimental research on an obscure problem, and it is small wonder that with his lack of

knowledge of parasitism he failed to identify bunt with a parasitic fungus. He did, however, definitely prove not only the value, but the necessity, of the experimental method in research, and established forever the fact of the infective principle of bunt dust. Richly did he deserve the great honor accorded him by the Academy of Science of Bordeaux.

HARRY B. HUMPHREY

DISSERTATION ON THE CAUSE OF THE CORRUPTION AND SMUTTING OF WHEAT IN THE HEAD

BY

MATHIEU TILLET

TO THE KING

SIRE,

A signal distinction was, then, reserved for me, when one of your Academies favored and honored me with its votes for the second time. YOUR MAJESTY deigns to welcome my slight accomplishment and, by appearing under his auspices, permit it to be announced with that éclat hardly merited by writings of the first order.

The exceptional privilege you have accorded me I doubtless owe, SIRE, to the approbation of an enlightened group and to the important subject it has furnished me.

Occupied with the responsibility of serving your happy people, you make us all mindful of this great objective: useful researches interest you; the slightest undertakings that tend to benefit the public are supported by you; and this unmixed pleasure, which one experiences in service to mankind, is unquestionably a part of the pleasure of serving you.

What say, I, SIRE? Agriculture, the foundation of our riches, and the most precious of our resources, becomes for YOUR MAJESTY the subject of ceaseless solicitation; you examine its principles; you are the august Spectator of Experiments designed for its perfecting; and, without losing anything of the grandeur of the Monarch, you perform with zeal the simple functions of the Citizen.

ROME and FRANCE, in their best days, posterity will tell us, saw labor succeeded by victories and triumphs; the plow was guided by heroes. CINCINNATUS quit the purple and

returned to his fields, wearing a crown of gold, which the Roman army had bestowed upon him. LOUIS sometimes descended from the throne, and, crowned with laurels, presided over the culture of the soil.

I am with a very profound respect,

SIRE,

OF YOUR MAJESTY,

The very humble, very obedient servant and very faithful subject,

TILLET.

PRELIMINARY DISCOURSE

THE different contributions that I bring together to-day under the eyes of the reader have been presented to the Academy at different times, and without its having been possible for me to express myself like a celebrated Writer by presenting my work at a single stroke. This Society, interested only in demanding research of concern to the public weal, proposed in 1750 the subject of this dissertation; it was absolutely new to me: I then felt that I should first examine how it had been treated up to that time, reserving to observe for myself wheat plants in 1751 and to follow them in their various stages of growth.

This first task led me to some useful information; but, still, it but meagerly satisfied me from the standpoint of my main object. In the hope that, through different experiments to which I could wholly devote myself, some light on the origin of the diseases of wheat might be revealed to me, I hastened, from the month of October, 1751, to take this instructive and exact method of compelling nature to explain herself.

But the time of the meeting expiring by the last of April, 1752, I was in a position to present to the Academy only the first part of this work, in the which my experiments were merely announced; the result of these same experiments being presentable for communication only after harvest and too late for its [the Academy's] judgment.

To this first part I presently added several observations in the form of a supplement. I had made these observations on my seed plots while the wheat plants were still standing and before I clearly saw the actual objective I sought to attain.

I awaited with impatience the decision of the Academy when it proposed anew the important subject in question, and invited the authors who had transmitted some memorials on the subject to add some new observations.

Consequently, I sought to confirm the results of experiments presented in the first part of this work; I repeated in 1752-3 those that had appeared to me to be decisive. I devised new ones; and, withal, laid out in order, I initiated a series of experiments relative to the two contributions that the Academy already had in hand.

This assemblage of various facts, I believe, related in great detail and accompanied wherever possible by considerations, became naturally the second part of my dissertation, and is, without question, the most interesting, as a whole, and the most valuable to me. To it I owe the distinction and approbation with which the Academy has honored me. It is of other plaudits, those of the plain husbandman, which, although less impressive, I would be most heartily appreciative should I be so fortunate as to win them. I have labored for this class of people, so dear to the State; I have occupied myself in their interests, that they, themselves, might be alert and gain from the fruits of my labor; I shall find them grateful if my researches profit them anything; any attention they may give them I shall regard as an eulogy; and I shall receive through their peculiar benefits the reward I most covet.

The contributions thus separately made are, then, reassembled for one and the same reading, in such manner that the last shall serve ere long to reveal the mistakes overlooked in the first. These contributions, not articulating with each other, in an exact manner, present an irregular and interrupted succession. They do not at all form that continuous thread, that chain of ideas that follows a well-coordinated plan without interruption.

But, as has been observed, this sort of imperfection in a work has something instructive about it; it presents naturally the progress of human thought in a new or little-known subject. It shows at first the readiness of the human mind to seize upon anything bearing the appearance of truth and its hesitation to reject completely a system that had baffled it, its subsequent discretion in the presence of false appearances, its ability to catch a truth-revealing gleam of light, its zeal in dissipating the clouds that still envelop it, its service, finally, in revealing the truth to the full light of day and in rendering it as evident to others as it truly is. A philosophical mind sometimes finds material for reflection in

that which appears to the common man hardly worthy of consideration.

The first part of this dissertation is divided into four chapters; it would have been tiresome for the reader without these moments of rest when he might catch his breath and relax his attention; I have even subdivided the third chapter, since it is more weighty than the others, and requires a particularly clear and equally distinct presentation of views.

CHAPTER I

The first chapter is intended to summarize what different authors have said on the origin of the principal disease of wheat and on the remedies that should be applied. The scant agreement that obtains between them and the marked opposition one sometimes finds will be apparent. Sometimes the source of the disease is regarded as independent of the seed and the treatment of the seed rejected as useless; sometimes this source of corruption is not considered as at all foreign to the seed and its treatment has been adopted as a protective measure; here certain manures are excluded as dangerous; again, the whole is indifferently admitted as harmless; certain ones fear for the wheat the malignity of certain fogs; others pay no attention to them and fear only failure of fecundation, or an excessive humidity or a violent sunstroke on the water-soaked heads.

CHAPTER II

It was necessary before discussing the various opinions advanced on the cause of the disease of wheat to cite first, as so many principles, some observations on which one may depend; to present facts that nobody can question, in order that those opinions, compared with some established facts, either stand on their merits or fall outright. The reader will notice in the second chapter, how, in following this method, the most accredited opinions are discarded without one's being able to reestablish them on any sort of foundation.

CHAPTER III

It was still necessary to present in their order the different diseases of wheat that had up to this time been confused; to distinguish the less dangerous from those one should be the most disturbed about; to use, in naming them, the correct and characteristic terms; and to describe them with enough care to make it easy to recognize them. Somewhat has been added to chapter three, namely, a particular digression has been made on the disease of rye, which, generally, is known under the name of Horn or Ergot of wheat, and

there has been given there an explanation of the malformation of kernels attacked by this disease. The reader will note in the second part of this work that the facts agree with the explanation and that the sting of an insect is the real cause of ergot.

After a great number of observations on the nature of troubles peculiar to wheat, I recognized that one sought in vain the cause, whether in the meteors, or in the failure of maturity of the seed, or in the kind of manure with which one fertilizes the soil; and I believed, in fact, that I saw coming out of a matter so obscure some gleam of truth when I discovered the insects that live on wheat. These animals, always very small, and nearly imperceptible when they are young, belong to the class of Staphilinids [Staphilinidae]. I examined them with such attention and such secret joy as a new object can engender, and whence appeared to depend the issue of a great difficulty. I neglected nothing that might instruct me as to the precise point of their origin, the manner in which they were nourished, the kind of damage they were capable of making, and the relation this damage might bear to the diseases in whose origin I had so much interest.

CHAPTER IV

In spite of the attachment that it is natural to have for new ideas, I never closed my eyes to what was not clear to them; the love of the truth made it difficult for me to censor my own system, and after having long regarded these insects as the possible causative agent of the major disease of wheat, I finally considered them such no longer and yielded to an hypothesis as hazardous as those hypotheses I had contested.

Perhaps I should have reduced somewhat the material of this fourth chapter, since it leads to nothing profitable on the origin of the ruined grain; but it occurred to me that certain others may be as forcibly impressed by the insect hypothesis as I was; I feared that this somewhat specious hypothesis would eventually have prevented them from correct conclusions had I not prepared them by revealing the futility of my observations and the little progress I have made, notwithstanding my persistence in following these insects, in a study of their behavior and in an effort to find the germ of the diseases of wheat within the refuse they may produce.

The correct method of preventing mankind from falling

prey to an illusory system lies perhaps less in methodically, and by the simple process of reasoning, revealing to them the spurious than by letting it be known how we ourselves have been attracted by it, have considered it in all its aspects, and, with something akin to regret, have finally abandoned it as incapable of proof. Further, one feels somewhat humiliated, as is always the case, when one has made a mistake and has fallen prey to an illusion; one enlightens others without wounding their pride; and still leaves them the honor of thinking that they had perhaps seen at first glance the entire fallacy of the system against which they had been warned.

In assailing in the second chapter, the various opinions that I have compared as to the cause of the diseases of grain, I did so only in support of my own general observations and from naturally drawn inferences. I soon realized that after devoting myself to helpful speculation it was still necessary to have recourse to experiments, to put my hand to the task, and reply to all by the facts disclosed before my eyes. I, therefore, made in the month of October, 1751, different sowings the details of which one may see at the end of the first part and the result in the second part of this dissertation.

After the wheat had attained a certain height, I recognized more than ever the extent to which tests, supervised and followed scrupulously, nullify false ideas, clarify present new views, and teach one to hazard nothing in judging the secret operations of nature. In the supplement of Part I will be seen the first fruits of my experiments; how I had to consider, with the greatest care the three principal diseases of wheat, to determine several characteristic facts with regard to each of them in particular; how I came to doubt, with good reason, that they had a common origin; was obliged to contend more than ever against the opinions I had challenged; and finally, to retrace my own steps, in giving up the thought that the insects already referred to were the inciting cause of any damage, from which the diseases of the crop had arisen. Moreover, the reader will find there the observations I made on the roots of diseased wheat, which were for a long time the object of my studies; I noted in them some changes; I suspected the insects of being the cause, and believed that I perceived some relation

between these changes and the diseases of the grain. So true it is that an idea with which one has been seized and that one has too inconsiderately accepted as a first ray of light, offers itself without ceasing to the mind, although it already may have been rejected, still pleases under a new aspect and seems to yield to the inclination of the imagination it has impressed. This supplement ends with a presentation of the cause of the disease of the kernels of wheat by showing in prospect the facts that the second experiments disclose.

The second part of this work is, in the main, to be considered only as a complex of experiments, a naïve exposition of the procedures followed, of the results that they gave me, of the deductions I drew from them, of the objections that I foresaw, of the answers I conceived. I do not at all, therefore, prejudice the reader in presenting to him this abridged part. It would be difficult for me to reduce it advantageously to some principal facts; it would appear dry, meager; the account I would make of it would have no grace, no expression; it would be better that one get no fixed idea of it before reaching it. The first experiments will prepare for and require those that follow; the dependent truths will follow in immediate sequence; the cause of the disease will be disclosed; the remedy will be anticipated with something like impatience; this cause, having itself a double origin, will become readily perceptible; the remedy will be wholly effective; and the reader, finding himself finally at the end, will be inclined to believe with me that the experiments were conducted with care.

DISSERTATION
ON THE DISEASE THAT RUINS THE GRAINS OF WHEAT

IN THE HEAD AND CONVERTS THEM INTO SMUT;
And on the Methods of Preventing these Untoward Circumstances

THERE sometimes is complaint that the subjects of Academic prizes are of no interest to the public, and seem designed rather to exercise the talents of the man of the study than to procure a substantial benefit to society. Here is a subject that surely will not be confused with those of pure curiosity; it concerns the people generally; it impinges upon their interest directly and merits, in many respects, the attention of the best natural scientists. But, although from the viewpoint of natural history it offers something of interest and practical value, it presents many more difficulties than one might believe.

At first glance, to comprehend the nature of the particular disease of wheat in question does not seem to require arduous labor; it seems to be self-evident. But, when we consider the malady from the standpoint of its cause, it becomes more obscure; the disease is before us in its entirety: It has not been possible to set out from a known point of origin in order to follow the progress of the destruction of the kernel. The derangement of the organic parts of the plant has taken place insidiously; and, even though one recognizes it finally by certain external symptoms, it is then too late to disclose its cause.

After all, we need not be surprised if the diseases of plants have their obscurities. The admirable mechanism of plants is almost entirely revealed before our eyes. The thousand peculiarities that relate to their internal configuration, the action of their parts and their successive growth are unknown and will, according to every indication, remain so. One is still in doubt as to the exact movement of

the sap, which fills and nourishes them. The complete function of their parts is not at all perfectly known; the diseases without number to which they are subject have been but little studied. Why flatter ourselves on the easy recognition of the causes of their diseases, the application of suitable remedies, without having considered them carefully and without having made a special study of them?

The diseases of the human body have been, throughout the centuries, the subject of the most profound research. Many brilliant men have shed light on this essential part of natural philosophy; medicine to-day has attained a high degree of perfection; yet, how many things are unknown in the field of animal economy! How many diseases there are of which we fail to recognize the cause! How many remedies of which the success is doubtful!

Let us not hope, then, for an intimate and immediate acquaintance with the contingencies to which plants are subject. Let us accept as a fact that a clear understanding of the disease of wheat that has been made the object of this memoir requires much research and let us consider the degree to which it is veiled in uncertainty as to the provoking cause, even though the disease be everywhere common to wheat, and even though celebrated men have given it some attention.

Before giving an account of my work, and proposing new ideas as to the disease that ruins the grains of wheat in the head, and blackens them, it will be well to present the scope of this memoir, and to show, in the outline I have made of it, the exact procedure I followed in order to disclose accurate and fundamental facts.

First, I shall now recall as succinctly as possible, and with all that fidelity that is by right expected by a distinguished Society, all that has been said, and that is plausible, concerning the subject proposed by the Academy.

In the second place, I shall show by thorough proof, and through my own experiments, how the diseases to which one commonly attributes the destruction of the seed are attended by a multitude of obscurities, several of which appear inexplicable; and how the remedies employed, and in consequence of the assumed cause or causes, do not go to the seat of the trouble, at all.

Thirdly, in order better to characterize this destruction

and smutting of the seed, which I ought first of all to do, I shall present in detail the diseases of wheat that have seemed always to have been confused under the too general names of smut, blight, black wheat, etc. I shall differentiate them by certain peculiar characteristics; I shall at times show a sort of liaison between them by attributing to them the probability that they may have had a common origin; and I shall compare with these same diseases of wheat those that are slightly similar and known to occur on other plants.

Fourthly and finally, I shall give the details of my own observations on kernels of wheat ruined in the head and reduced to a blackish dust. They will be designated in due course under the name of carious, smutted kernels. I shall propose some conjectures on the primary cause of this disease. I shall advance nothing that I have not seen with my own eyes, and that, too, several times. I know by experience how much one runs the risk of being deceived in matters with which he is concerned when he does not speak in accord with those whc have considered them, or when one adopts too easily the discourse of even the most intelligent farmers. I shall not defend the doubtful in my observations; I shall lay stress upon whatever will have appeared to me to be conclusive; and I shall take care to offer nothing as certain that I have not consistently noticed in the different Cantons.

CHAPTER ONE

FIRST PART

Diverse opinions on the disease that ruins the grains of wheat, and smuts them. Means that have been believed capable of preventing these unfortunate results.

THE newspapers are commonly enough the vehicle employed in demanding light on some interesting point or to announce a useful discovery. The cause of the corruption of the grains of wheat in the head, and the remedies one may apply deserve certainly to be studied with care. This subject is of a nature designed to impress the minds the least qualified to inquire and seems to require that every citizen give it his attention. Therefore, the Journal of Verdun, which is quite generally distributed, has several times been appealed to for information on the cause of the wheat rot and to instruct the public on observations made thereon.

Let us pass in rapid review the references in this periodi-

cal wherein the subject in question has been touched before we pause to consider in more or less detail the statements of recognized contributors. These references, it is true, furnish few facts; with difficulty some traces of light penetrate the mass of snap judgments and chance observations; but, meager as they are, I believe they should be assembled: the record of errors, moreover, has some advantages; it presents to us a thousand mistaken ideas, and serves to introduce the truth.

One finds in this journal a letter that describes quite fully the disease of wheat with which it treats. An error, not at this moment to be disclosed, has, however, crept into the account. Journal de Verdun 1730. January, page 31.

Subsequently, the manner of treating the seed is indicated, which deserves some attention. If one doubts that the recommended treatment is as beneficial as we desire it to be, it at least is a convenient and perfect method of washing the seed. Change of seed is prescribed here as a precaution often taken by the farmers of Picardy, and one the usefulness of which they have recognized. 1739. October, page 244.

In 1739, a great part of the wheat in the Electorate of Chattellerault, was attacked by smut. The author, who noticed it, carefully observed the symptoms of the disease in their every aspect. The dangerous mists, which fall on the wheat when it is in flower, appeared to him to be the only cause of the disease; he saw no danger in sowing smutted seed; and every method of seed treatment seemed to him useless.

The system changes. The farmers of the Caux country, persuaded that the same seed degenerates in soil of the same kind, and that the disease of the grain may result through the use of seed that has run out, have taken care to obtain their seed from a poorer country than their own, and above all to avoid sowing contaminated seed. 1739. December, page 437.

Here is an opinion quite different from what I have presented. *Idem.* Some say wheat is ruined by the moisture absorbed by the grain put aside in the barn for seed. A sovereign remedy, the details of which have not been made known, has been reported, a remedy that invigorates and increases the resultant plant growth, apart from any other amendment or salutary agent. Some fields of wheat, subjected to this treatment, have become very excellent,

though sown in the month of March, and have produced at least as much grain as fall-sown fields.

This treatment, if repeated experiments establish the surprising effect, should be a great boon after a winter as rigorous as that of 1709. It would assure the success of second sowings, without which treatment all would be futile, under the temperature and seasonal conditions that so commonly prevail in France. M. Duhamel demonstrated this by sowing wheat in February and May; he did not succeed. I made the same test in March, with as little success.

Mén. de l'Acad. des Scienc. 1746. Pages 76 and 77.

Could it not probably have been due to particular circumstances, in combination with several causes favorable to the growth of wheat, but rarely experienced in the months of February and March, rather than to the kind of seed treatment, that the success of the March seeding was due? The order of the seasons does not permit the sowing of wheat in Canada before spring, and even then not before the end of April, in regions in the neighborhood of Quebec. Wheat there, however, comes up, ripens, and brings its heads to full maturity in the space of some months; the harvest ordinarily occurs there toward the end of the month of August.

Mém. de l'Acad. des Scienc. 1746. Pages 92 and 93.

The author of the reported opinion again asserts his adherence to it, and adds that even the imperfect maturity of the seed may be a cause of taint. The remedy presented in the foregoing prevents, says he, this latter trouble, because it is a preservative of the seed against humidity. It will be seen in due course how this author is deceived concerning the real effect of his preparation; but, without knowing the cause of the disease, he observed useful precautions.

1740. Février p. 14.

Page 117. In the same journal one is advised to use arsenic for the treatment of the seed, and change of seed is spoken of as being helpful.

Idem. Different memoirs are cited there on the disease of wheat, from the summary of which it appears that one of the better methods to which one may resort in order to avoid the destruction of the seed is to abstain always from sowing a given kind of seed in the same soil, to sow only healthy, perfectly dry, and mature seed.

Idem. Farther along, one is instructed against the use of arsenic

in the preparation of the seed, and the serious effects that have followed its use are described.

Common opinion appears here in a new phase and supported by some plausible reasoning. A physician, moreover, very well informed, sees no advantage to be expected from the different seed treatments other than a more prompt germination. The cause of the disease lies, according to him, outside of the grain. It is a purely ethereal, volatile oil and, moreover, viscous, which falls on the plant, even in fair weather; he calls it *Enmiélure*. This material, activated by the sun, becomes instilled into the substance of the most delicate bodies. It alters the still tender fruits, the leaves, and sometimes the young shoots of trees. The kernels of wheat, which it penetrated while they were still in the milk, are arrested in their development, become diseased, and, one might say, gangrenous. The different stages of growth in which the grain may be when this material penetrates them result in different manifestations of the disease. From the observations of this physician it is obvious that the *Ebrune*, the term used by him to designate the dust of smutty wheat, is a disease one may try in vain to remedy. 1741. July, page 22.

Another word concerning a new opinion and I close these extracts. The author assumes this attitude: He cites the experiment and assumes that sheep manure, spread on soil that has just been seeded, is the cause of the bad condition of the seed. 1751. May, page 365.

This opinion is promptly attacked. It is tested by experiment after experiment, and it is concluded that the heat of the manure is not at all the source of the disease with which we are concerned. 1751. September, page 205.

The authors of different treatises on plants, or on agriculture in general, have considered the particular disease of wheat we are examining. They have sought the cause of it, they have stated their opinions; but always they have left the subject veiled in almost as much obscurity as it was in before they had reflected upon it.

Without referring back to authors of remotest antiquity, it will suffice here to cite Jean Bauhin, celebrated botanist of the last century, who ignores nothing in his writings and who reports the opinion of Theophrastus and of Pliny on the origin of the corruption of wheat, after having presented what he himself had thought concerning it. *Hist. Plan.* *tom*, 2, page 418.

He said that, ordinarily, one could perceive the contamination of the grain when the head emerged from the stem [boot]. He attributes it to the action of the sun, the burning rays of which fall at intervals on the head that an abundant rain has penetrated; he assumes that the leaves and stems do not die at all, but that the kernels change into a blackish and sooty powder.[1] This botanist subsequently reports on the origin of these black kernels the opinion of Theophrastus, which differs little from his, and seems to be the basis from which he drew his.

Theophrastus regards as very essential for the health of wheat the situation of the piece of ground where it grows; he assumes that, when the wheat is in low places and little buffeted by winds, the rain water lingers in the heads and becomes a source of injury as soon as the sun strikes them.[2]

Theophrastus adds that wheat is especially damaged during the full moon and gives a reason for it, the fallacy of which none of his contemporaries knew; that is, that the moon may heat substances and occasion, by its heat, injury of the heads during the night, just as he assumes that the sun may contribute to the same effect during the day.[3]

Jean Bauhin, after having reported the opinion of Pliny, who said nothing decisive on the point under discussion, finishes by designating the months when wheat begins to show impairment and returns anew to the cause to which he attributes the smut of the grain.[4]

The celebrated Mr. Wolf, in his book[5] on the multiplication of plants, devotes himself at great length to the cause of the destruction of wheat: the manner in which he depicts

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[1] *Ustilago, ubi in spicam abit culmus, apparere solita, si pluvia madens, incert folis sentiat fervidiores radios; culmo foliove neutiquam degenerante, sed granis in fuscum, fuliginosum pulvisculum commutatis.* Theophrastus.

[2] *Nec segetum situs (according to the translation of Gaza) naturaque parum refert: tractus enim excelsi perflatique rubiginem nullam aut minimam sentiunt; concavi & flatibus nullis expositi contra frequentissimam habent . . . spicam inclinari utile est ut imbres decidant, nec rores excipi possint . . . Segetes vento expositae minus aerugine infestantur . . . Elatus enim humorem dejicit, & sol, paululùm immoratus, nullam putredinem conficit; sed, priusquàm emarcescat humor, totus assiccatur atque absumitur: aerugo autem quaedam putredo est; nec putridum esse quicquam potest fine alieno calore.*

[3] *Ratio in calore est quo etiam noctu luna putrefacere potest.*

[4] *Caeterum nascitur ustilago Aprili & Maio mensibus, cùm coeli tempestas crebrò inconstans aliàs nimbosa aliàs serena fervens fuerit: tunc enim spicae humore madidae à solis fervore corrumpuntur, & quodam modo, uti aiunt, aduruntur.*

[5] This book is written in German: the Actes de Leipsic rendered the title in the following words: *Vera causa multiplicationis Frumenti admirandae:* and provided an abstract of it in 1718, page 178. Manget reports it in its entirety, Vol. 4, page 644.

it is, nevertheless, a little confused and he is obscure as to the kind of disease he has in mind. He does not state, which is nevertheless essential, whether the grains of wheat, in becoming internally converted into a black dust, retain their epidermis and their essential shape; he seems only to wish to speak of a malady to which wheat, barley, oats, etc., are exposed by employing a general term, which amounts to that of blast. He does not distinguish, as we shall judge later on in this memoir, the particular disease with which he is concerned from the one that affects the head as a whole. We shall speak of it ourselves in the article where it shall be a question of characterizing the principal diseases to which wheat is subject.

An appended extract from the chapter by Mr. Wolf, on the smut of wheat, would fatigue the reader; I am limiting myself to observing here that this savant, after having discussed the opinion of different authors on the subject in question, concludes by regarding as laid bare to its foundations the causes to which it is customary to refer the smut trouble and finally gives an exact account of his own observations. They led him to construe the cause of smut as a sort of monstrosity, a faulty conformation of the grain. They have led him to believe that the nourishing sap, which moves in plants, becomes contaminated in the vessels of the smutted kernels; because these vessels, being of an aspect contrary to nature, stop within themselves the course of the sap, and thereby become a cause of disease.

The author of the *Spectacle of Nature*, in an interesting detail on seed, particularly wheat, on its germination, on its growth, and on the practice of different sections, has not failed to say a word on the disease to which it [wheat] is subject and, like a careful observer, all but explained the manner of judging what he examined.

"Everyone knows," says he, "that rust[6] appears after an extremely light rain and is followed by a burning sun; when these raindrops rest on the stem, they become there so many little burning glasses, which burn, depress, and blacken the stem at so many points." Tom. 2, page 314.

[6] One sees here how necessary it is to establish the meaning of terms one employs in describing the diseases of wheat. M. Pluche seemed to mean by the word *Nielle* only the alteration of the stem, while others understood it to mean the destruction of the kernel.

"Drizzling rain does not prevent wheat from growing as does black rust; but it converts the kernels into a black and putrid powder. In examining, last year, several blasted kernels, I found them without a germ, and I perceived almost always, either laterally or underneath each kernel, the two flowers, which, having failed to mature, to eject their pollen, had remained there without furnishing the kernel the principle that develops and perfects the starchy content with which the grain is filled. Such is the disease; but who will furnish us the remedy?"

Page 211. M. Duhamel, in his excellent treatise on land cultivation, tells of different diseases to which wheat is subject, and presents them in a very methodical order. He first distinguishes under the expressive names of "rusted wheat," "rickety wheat," "scalded and shriveled wheat," and "wheat invaded by insects," the diseases known in England by the name of wheat blights. He assigns them to separate classes in setting forth their more or less varied consequent effects; he traces only the causes of those diseases that appear the most natural and passes on to the examination of
Page 224. the disease that is the subject of this dissertation. He considers it, apart, as more important than the others and designates it under the name of black, or smutted wheat; let us consider briefly what he has written about it.

After having described this diseased wheat, such as we have already characterized, he says that black wheat does not germinate. He assures us, contrary to the opinion of Mr. Tull, that the sound kernel, the little brush end of which has been blackened by smut dust, has in itself no tendency to produce smutted wheat. He avows that one does not yet know the cause of this disease. He appears to be convinced that smut is to be feared if much cold rain follows during development of the head. He sets forth the opinion of Mr. Tull, who thinks it is not the water that falls on the stem, the leaves, and the head that damages the kernel, but that it is too much soil moisture. He cites the following experiment performed by Mr. Tull in order to support his opinion: We shall soon see the justice in M. Duhamel's wishing that it might be repeated.

"Mr. Tull dug up some wheat plants in a field and
Page 227. planted them between two casements of his room, in a vessel full of water, or at least in some very wet soil. These

wheat plants produced some heads, but all of the kernels of these heads were black and smutty, although in the field from which he had taken the plants there was no smutted wheat. Evident proof, says Mr. Tull, that this malady resulted from the excessive moisture present in the soil, and not from that which wets the heads and stem; for no water had fallen on the wheat that was between the casements of his room."

M. Duhamel, far from regarding this experiment as conclusive, noticed, on the contrary, that he sometimes found as much smutted wheat in the highest parts of sown fields, where there was little moisture, as in the lowest of these same fields, where the water was obliged to stand. This learned Academician finished by referring to Mr. Tull's method of treating the seed to protect it against smut. An accident, concerning which it will be necessary to read the author's account, furnished the idea for it, a treatment that consists in sprinkling the wheat with a strong brine of sea salt, by dusting with well-pulverized quick lime, and by stirring well afterwards in order that all the kernels be equally exposed to the mixture. Mr. Tull, on the testimony of a farmer in whom he had confidence, speaks also of a change in seed as being helpful. Besides, the English naturalist assures us that since he began cultivating the soil in accordance with the new method he has had no smut in his wheat. M. Duhamel, who has followed him with this kind of practice, as one knows, has not been so fortunate in this matter. He says that this method (otherwise excellent) does not guarantee wheat against smutting; and that one *arpent*, cultivated according to the new rules, contains almost as much smut as another *arpent* in the culture of which these principles have been omitted.

Expér. relat. au Traité de la cult. des Terres Page 20.

In the splendid review of the memoirs presented to the Academy of Sciences by various savants, there is one very remarkable memoir on the growth of plants on other media than the soil. M. Bonnet, of the Royal Society of London, Correspondent of the Academy and author of this memoir, expounds on the cause of wheat blast. He believes we should attribute it to cold dews, which the first rays of the sun activate. He assumes that wheat plants growing in elevated places, like the mountains, are less subject to this disease than are those of the level country, and especially less so than those in low and moist places. He states that

Mém. de Math. & de Phis. tom. I. 1750.

Page 431.

one observes much more rarely smutted kernels on bearded than on beardless wheat. He gives as a reason for this the fact that the beards doubtless shield the kernels from the dew and prevent it from clinging to them. He brings to his opinion everything that he believes able to favor it, but asserts, nevertheless, that his observations should not be regarded as conclusive, since he has not made a subsequent study of the particular disease of wheat with which he was concerned.

CHAPTER II

Observations, certain facts in the light of which one examines the opinions presented in the preceding chapter

BEFORE adequately summarizing the different memoirs that I have already presented for analysis, before destroying the majority of the conceptions that have been advanced, in order to square everything with the opinion once adopted, it appears absolutely necessary to consider the too-much neglected practice of amassing a certain number of well-established facts and of speaking only in accord with exact observations. Although some of the observations that I have presented had already been made, I have, nevertheless, verified them with care; one may regard them all as the fruit of scrupulous study. I shall regard them, moreover, as so many principles to which I shall always refer, whether to judge of what will have been said by various authors, or to adjust and fortify my own ideas.

FIRST OBSERVATION

The black and blighted heads borne by the different culms, but deriving their roots from one and the same seed, are, ordinarily, all black and spoiled.

SECOND OBSERVATION

One and the same seed produces sometimes good and bad heads; a given head bears healthy kernels and blasted kernels; a portion of the head is sometimes ruined and another part is not: a bad kernel sometimes is found between two good ones, or a healthy kernel between two bad ones.

THIRD OBSERVATION

In the glumes that enclose the black kernels one finds consistently the three stamens; they have adhered to the kernel; their tops extend one-half to two-thirds the height of the kernel: They sometimes rise to its full height; but they rarely exceed the height of the kernel.

FOURTH OBSERVATION

One often finds blasted heads even in the sheath, although the sheath may be green, completely closed and leading one to suspect no contamination.

FIFTH OBSERVATION

Bearded wheat, as well as beardless, is subject to blast.

SIXTH OBSERVATION

Kernels of rye, although very delicate in their early development and subject to a particular kind of disease, are never converted into a blackish dust as are kernels of wheat.

SEVENTH OBSERVATION

A wheat plant, purposely placed in a constantly impoverished soil, produced many heads in which not a single kernel was found that was black and ruined.

In the first memoir, taken from the Journal of Verdun, the author offers nothing concerning the origin of the disease of the kernel; he is content merely with describing it. The author of this memoir deceives himself, according to the third observation, when he states that the blasted heads blossom like the healthy ones. The reply to this memoir adds nothing as to the cause of the disease; it presents only some palliatives such as the selection of seed, a preparation comprising various ingredients, and a change of seed. There will be no question for the moment as to the advantage or the usefulness of seed treatment prior to sowing: We shall speak on that later, along with the custom of changing the seed, practiced by some growers; and we shall see how one and the other precaution do not go directly to the source of the trouble, or at least remedy it only in part, and seem founded on some known benefits.

In attributing the cause of wheat blast to the mists that obtain while the wheat is in bloom, and that enter between the glumes when they are half open to permit emergence of the stamens, one apparently is deceived, according to the first, second, third, fourth, and sixth observations. Deceived according to the first, because it would be singular that several heads, all derived from the same seed, should be ordinarily all spoiled by the mists, while other heads belonging to another seed, showed none at all, although the

stems of these latter were often mingled with those of the first, and grew in such manner that the healthy heads were alongside the diseased ones. According to the second observation, one is apparently deceived. How, in fact, can one explain by the influence of mists this mixture of good and bad kernels, which one finds sometimes in the same head? The third also is wrong because stamens, never rising above the kernel, are unable to open the glumes and give access to the mist. According to the fourth, we are deceived because the head is often blasted before the sheath opens. Finally, in attributing this malady to the malignity of mists that intercept a scarcely perceptible, tender, delicate grain, how can one adjust this to the sixth observation? Are not the kernels of rye in their incipiency extremely delicate? Are not mists more common and more dangerous while rye is in bloom than when wheat is in that condition? Rye, however, is never subject to the effect of mists or, at least, allows nothing to bring about the effect that is attributed to it in wheat.

In *Pays de Caux* the farmers attribute the cause of wheat blast to seed that has never been changed, or to smutty seed of similar history. Consequently, without concerning themselves with any external cause, they consider the seed placed in the ground as becoming the one source of the disease because of its internal imperfection or because of some exterior defect, even before it produced a crop. But, it will be said, in supposing the kernel vitiated in its beginning, or in a purely accessory manner, how, following the second observation, can it sometimes produce good and bad heads, good and bad kernels? This objection is without doubt a formidable one, and I agree that I was at first puzzled; but I have finally recognized, through some carefully conducted experiments, and of which I shall give the details, that contaminated wheat kernels produced smut in abundance and produced it with the inconsistencies that give occasion for the objection.

The author of the following opinion, in giving as the cause of the disease lack of maturity or dryness of the seed, and in adhering to the custom by which only well-ripened and well-dried seed is employed, will not explain thereby the origin of a disease which, carefully examined, is peculiar to wheat, to rye grass, and perhaps to some other plant. The

cause that he indicates is general: if it were the real source of the trouble, it would be evident from time to time in all the different kinds of grain and would produce therein the effect that the author says results from it. No more attention is given to the ripening and curing of rye than is given in the case of wheat. The same farmer who harvests sometimes badly damaged wheat always harvests rye that is not, or that is not damaged in the same way. Would this farmer fail to give attention to the most valuable of all the cereals; would he neglect only the seed of wheat? We would prefer to state that the damp and imperfectly matured kernels will perhaps drop prematurely, or become diseased, but they will not show the peculiar effect that we have under consideration.

The physician whose system we have presented, will have some difficulty in reconciling the majority of our observations. In attributing the disease of smutted grain to an ethereal, volatile, viscous oil, which the sun activates, he gets himself into a multitude of difficulties the outcome of which I have not been able to determine. Several of the reasons I have given in the foregoing, in order to exclude mists as a primary cause, are equally opposed to his opinion and should be regarded as rendering it essentially insupportable, if they be sufficiently sound to warrant their adoption.

Those who assert that wheat, sown in a soil immediately after having been spread with fresh sheep manure, runs the risk of becoming smutted are apparently deceived, according to an experiment that I shall have occasion to report. They attribute the disease to an alleged excessive heat, which affects and alters the kernel. The reference I have made to the subject of contaminated wheat kernels that produce a great quantity of smut and, sometimes, transmit it only to certain heads of a given stool, impels me to offer an idea that I, at first, believed capable of refuting the opinion I here attack, that a kernel of wheat, originally defective, ought either to rot in the soil without germinating or produce only a languishing plant or transmit generally and uniformly to all the heads it produces the more or less marked imperfection of which it is the source. But I shall ever be obliged to maintain, to the utter ruin of this opinion that, in order to damage the grain, a much higher degree of heat would be necessary than that supposed, in fact, to be pe-

culiar to sheep manure from the stables, but that soon had cooled on the soil because of the care with which it was spread in order to break it up into small portions; and that, even in the case where this burning fertilizer injured the grain by excessive heat, it was, according to my experiment, an injury that resulted in the complete desiccation of the germ and its consequent incapacity for further growth, rather than in inducing it to produce wheat plants from which rise sometimes as many as 8, 10, 15 very vigorous and laden stems, which, although contaminated, are very long and well filled with grain. In support of this reasoning I shall further add that the grain is not damaged by the common treatment with limewater, the heat of which, when one pours it on the grain, is much higher than that that would obtain even with finely divided manure.

The opinion of Theophrastus and that of Jean Bauhin no longer agree with several of our observations. They attribute the blight of the grain to the action of the sun, which strikes the head actively soon after an abundant rainfall has penetrated it, and they assume, in consequence, that the elevated and wind-beaten places are less likely to bear smutty wheat than are the low places, where the slightly disturbed and protected heads hold the moisture a long time. In the foregoing we stated that M. Duhamel had noted that the exposed elevated parts of fields sown to wheat contained as much smutty wheat as the lowest places of these same fields. First presumption against the opinion of those two authors. Otherwise, how would it be possible for him to return to the first, second, and sixth of our observations? Does the water activated by the sun act chiefly only on the heads that arise from the same seed? Is it to be presumed that rain penetrates extensively the heads that are completely closed within the sheath? Would rye, the kernels of which have never been observed reduced to a dust be more capable of resisting the joint action of rain and sun, considered so dangerous for wheat?

Mr. Wolf is persuaded that the sap, arrested in its circulation, this nourishing juice, stagnating, so to speak, in the false kernels that are like so many monstrosities, becomes there a source of corruption. In reporting the opinion of this scholar, we have said that he had not distinguished the two kinds of diseases of wheat in which the substance of

the seed is changed into a black dust: we shall soon supply this distinction: we shall give the details of a third disease, which he appears not to have known and of which I have not observed that any one has thus far spoken. The opinion of Mr. Wolf requires special discussion, and it will be better to await the exact knowledge of the three major diseases of wheat in order to discuss how these diseases have been distinguished one from the other. Then one will be in a position to understand, if one explains sufficiently, how, by the failure of circulation of the sap in the false structures and the desiccation of the malformed kernels of wheat that, according to every appearance, Mr. Wolf has ignored, it is absolutely impossible to apply the same reasoning to the wheat grains reduced to a dust concerning which he expects to speak.

The behavior of the affected stamens appears to be, according to M. Pluche, the cause of the blasting of the seed. It already has been seen that this opinion squares with our third observation. This author errs in stating that there are only two flowers, or stamens, beside the kernel. There are consistently three of them; and it is contrary to the natural order when one finds but two. Moreover, we shall show the agreement of this idea, resulting from an inspection of the kernel, with the ideas we shall present after a subsequent examination and to which we have for a long time adhered, in believing them more valid than they are.

M. Duhamel, author of the treatise on soil cultivation, who, unfortunately for natural history, did not study the cause of smutty wheat as he has applied himself with success to several interesting subjects, and especially to the perfection of agriculture, takes no particular interest in the cause of seed blight. It would seem that the cold rains that follow when the head is formed should, in fact, cause him to apprehend this unfortunate circumstance; but this opinion reverts to others I have presented in the foregoing and offers difficulties that are keenly felt to become greater in the light of our several observations.

Mr. Tull, based on the experiment that we have reported, believes that excessive soil moisture occasions the decay of the seed, and asserts that he has had no smutty wheat since he employed his new method, and that he has been careful to provide some furrows designed to facilitate the drainage

of the water. In order to cast considerable doubt on the validity of what Mr. Tull believes he observed, we would need only to cite M. Duhamel, who never noticed more smutty wheat in low, moist places than in elevated places, and who, in the experiment he conducted according to the principles of the English savant, was unable to guarantee his wheat against this malady. But I shall report here the experiment that I, myself, made in order to verify that of Mr. Tull; to me it appears decisive; I have felt that I should here give it in detail.

In the month of May I dug up a wheat plant, composed of several distinct tillers; it was put in an earthenware pot filled with common garden soil; this pot, the shape of which was that of an inverted bell, was 9 inches deep; its principal diameter, taken at ½ inch from the lip, was 11½ inches; and the minimum diameter, taken at 1 inch from the bottom, was 7¼ inches. The wheat was watered 4 times every day, regularly, until the beginning of August; each sprinkling amounted to a little more than ½ pint of water, so that the 4 waterings, taken together, formed about 2½ pints.

It is quite properly assumed that this plant, which was in a continually drenched soil and exposed to the south, should have grown with vigor, as, in fact, it did, in producing as many as 105 stems, of which one-third bore heads laden with grain, one-third bore some heads only partly filled, and the other third bore none save empty heads. But in this great number of heads I found none that contained diseased kernels, none whose kernels had been converted into a blackish dust; several heads were, to be sure, poor; a great number of kernels were shrunken; the majority of the stems were short and weak; but it was necessary to attribute these shortcomings to the too great abundance of stems within a very limited space, and to conclude, moreover, that, since so many heads, either in a state of perfection, or in a state of mediocrity, or in a condition of emptiness, contained no smut, the excessive soil moisture is not the real cause of the trouble. I should make note here in conclusion of my experiment that the stems and leaves of this wheat plant, nourished with water, remained quite green during the entire time that the wheat was watered, but by the beginning of August the water having been suddenly discontinued in order that the wheat might ripen perfectly, and

mature, the stems and leaves dried in a few days; and it is, according to every appearance, to this discontinuance that one should attribute the blighting of several kernels.

It remains for me to say only a word concerning the opinion of M. Bonnet on the origin of smutty wheat. He attributes it, with as little basis in fact as does Mr. Tull, to the humidity of low places, and believes that elevated places are less subject to it; he assumes, moreover, that the cold dews, which the heads have imbibed at the moment the sun flashes its first rays upon them, occasion the spoiling of the grain. M. Bonnet believes him, according to a remark that he expressed in due course and that deserves some attention. "It is observed," says he, "that diseased kernels occur much more rarely in bearded wheat than in beardless; the reason being, without doubt, that the beards hold the dew apart from the grain and prevent it from coming on contact with it." I shall be able, however, to assert here that, in the examination I have made of an infinite number of diseased heads, I have never been able to perceive that the beardless heads were afflicted more commonly than were the bearded ones. I have under observation a considerable quantity of heads, which I drew at random from several bundles collected in different cantons: In assembling them, I had in view only my special observations, and not the remark of M. Bonnet of which I was then ignorant. Since reading his memoir, where this that relates to our subject is only incidental and does in no way detract from the real value of the work, I have examined my little bundles of smutted wheat, and I have found in them at least as many bearded as beardless heads.

CHAPTER III

Differentiation of the Diseases of Wheat

ARTICLE I.

Description of the less dangerous Diseases of Wheat

I HAVE already stated in the course of this memoir, and the reader ought, himself, to have noticed it, that the authors, whose opinions on the causes of the disease of grain I have reported nearly always confuse, under a general name, all of the diseases to which wheat is subject, and assign to them a common origin. I have observed, however, that M. Duhamel distinguished with care several maladies to which wheat is exposed, and that he explained the different causes of them.

Rusted Wheat

He designated as *rusted wheat* that wheat the stems and leaves of which are covered with a reddish substance of the same color as the rust of iron. This substance, which becomes a very fine dust, is not absolutely adherent to the stem or to the leaves; it adheres to the hands on the slightest touch of the plant on which one has observed this reddish orange dust. The parts of the plant that are attacked by rust almost cease to grow; this disease, which causes sometimes a considerable crop loss, is commonly attributed to light mists, followed by bright sunshine.

This singular disease, rust,[1] requires an explanation. It

[1] The Romans recognized among several rural deities a god under the name of *Robigo* or *Robigus*. They celebrated, in his honor, feasts called *Robigalia* or *Rubigalia* and offered him sacrifices the seventh before the Calends of May, on the assumption that wherever they were he had the power to preserve their wheat from rust.

Nothing proves better how much of obscurity ruled at this time concerning this disease, and to what degree it has been confused with other untoward circumstances to which wheat is subject, than the terms employed by the ancient Latin authors and, after them, such moderns as Jean Bauhin and Longius in order to designate these diseases; terms that do not have the same meaning, that portray different symptoms, and that, nevertheless, these authors employ indifferently. *Ustilago, uredo, urigo*, which properly signify *burning*, and are rendered by the Greek word εμπνεισμος, often take the place of the terms *aerugo, rubigo, robigo*, which properly signify *rust* and are expressed by the Greek authors by the word ερυsιρω. Now rust of wheat certainly is not the same disease as two others of which we shall soon speak under the name of *smutted wheat*, and under the name of *carious wheat*. Trévoux's

doubtless would not be astonishing if certain mists, which one might conceive as being charged with nitrous and mordicant particles, become attached to the stem and the still young, delicate leaves of wheat, and changed them appreciably. But should one adopt the opinion of those who assume that this reddish substance, designated in some places under the name of honeydew, is absolutely foreign to the plant on which it is observed, and that it has been deposited there by mists? Should we not assume rather that the acrid particles of these mists have reacted severely on the stems and leaves, rather than that they have broken the tissue in some places and have caused the extrusion of a soft and oily sap, which has dried little by little, and become converted into an orange red powder. I have examined with a large hand lens several wheat plants whose stems and leaves were laden with rust; I have clearly observed that wherever the orange red powder was there also were some little cracks, and that the epidermis, either of the stems or of the leaves, was fractured here and there. I have observed this sap, reduced to a reddish powder, come out from these little openings, above which could still sometimes be seen the small portion of the epidermis, which, in tearing apart, imperfectly covers these little openings, and which one may compare roughly to that thin skin sometimes remaining on one's finger after an obliquely made cut.

In thus asserting that a thick and oleaginous sap escapes from certain plants, I am announcing nothing that experiment does not confirm, and that has not been previously noted by some competent observer. M. Reneaume, in a memoir devoted to the consideration of the nutrient sap of plants, expostulates at great length on sap exudation. He recalls that much already was known concerning the manna of Calabria, formerly believed to fall from Heaven, which is

Mém. de l'Acad. des Scienc. 1707. Page 276.

Dictionary, on the article *Rubigo*, also confuses rust with the disease of the kernels that are reduced to black powder or smut. These untoward conditions, however, are well distinguished in Holy Writ. Among the great number of evils with which Moses menaced the Israelites, if they did not obey the Commandments, he placed rust and the blast of wheat. The Hebrew word, which I translate here by blast, is rendered in the Vulgate by these words: *aere corrupto*. But one gives to it more commonly the sense I have here attached to it. The Septuagint has translated this same word by the term ἐνπυρισμός in one place in the celebrated prayer of Solomon where this religious prince supplicates the Lord to be touched by the cries of his people when famine, pestilence, the blast (of the wheat), the mildew [rust], and the ravages of the different insects will come to afflict them. Solomon knew from cedar to hyssop and, without doubt, was not ignorant of the various diseases to which plants are exposed.

Deut. Ch. 28. v. 22. Les Rois, Liv. 3. Ch. 8. v. 37.
בירקון Ustilago.
ירקון Rubigo.

nothing other than the extruded sap of the wild ash. He tells of a kind of manna extruded by the walnuts of the Dauphin, and of which the too abundant extravasation ordinarily causes these trees to die. Mr. Muschenbroek says that "after a very hot day, not only the fine and aqueous particles of the plants are found in great motion, but also the thick and oleaginous particles, which, being extruded from the excretory vessels of the leaves, without being able to return, are unable to evaporate into the air and, as a consequence, are obliged to remain on the surface of the leaves, where they form a thick material, like honey." One may see in the contribution by Mr. Muschenbroek that this opinion is that of a great number of observers and that it is based on some experiments that we are unable to accept.

Essai de Phis. Page 759.

Those who assume that rust is a deposit made by the mists alone can not explain satisfactorily why a certain stem is covered with this reddish substance, while none can be observed on a neighboring stem; for, admitting this to be true, the influence of mists should necessarily exert a general effect upon the more of less extended areas covered by them; and also because the stems and leaves should be affected quite uniformly by the fall of a thick, unctuous, and, without doubt equally dispersed material. Rather do they account for this unusual condition, i.e., of rusted wheat plants alongside of others that are not rusted, by stating that certain stems more delicate than others, and overtaken in particular condition, may be altered by mists, or such other thing as one might never suspect; that they may lose a part of their unctuous sap through points of injury, and, consequently, produce only poor and shriveled grain: while other more vigorous stems will resist the impact of the dangerous mists, will retain all their sap, and will bear excellent grain. That that happens in the order of animal economy seems to justify this explanation. It is observed, in fact, that there sometimes obtains in the air a malignity, that this contagious air penetrates our very bodies, that it occasions general, unfortunate circumstances; that many people, however, are immune from it, without being able to give any reason other than the particular condition in which they find themselves, and the nature of their temperament.

This detail on the subject of rust was necessary in order to show how they deceive themselves who regard it as the primary cause of the disease of wheat, and as an external agent that alters the wheat, while, within the wheat plant, it is the result of a disease the cause of which is not well known. This detail may serve also to enlighten those who believe that rust and this farinaceous dust that one notices on certain plants are accumulations of eggs deposited there by certain insects, and from which emerge a numerous family that is very baneful to plants. In accepting the opinion that I present to them, they will realize that rust, honeydew, powdery dew, and those fatty materials that one sometimes notices on the leaves of gramineous plants, of shrubs, and of trees, are the exuded juices, which gradually drying, become converted into an impalpable dust on certain plants, and change in color according to the species of the plant and the nature of the trees on which they are found. The exuded sap is yellow on broad beans, reddish on gramineous plants, and a pale green on the leaves of the plum.

Blasted wheat.

I return now to the distinction made by M. Duhamel between the less important diseases of wheat. He regards as *blasted* those plants whose heads, instead of being filled with good grain throughout their length, are absolutely empty to their tips, or contain only small kernels, which pass through the sieves and in which there is no flour. He believes that this condition may be occasioned by cold rains that sometimes fall at a time when wheat is in flower and that become an obstacle to fecundation. He expatiates also on the idea that lightning may cause the wheat to blight and speaks of extraordinary frosts that injure the head, emerging from the boot, either destroying generally all the grain, or causing only occasional damage.

Scalded and shriveled wheats.

Under the name of *scalded* and *shriveled* wheat plants M. Duhamel includes those that ripen without filling, that bear only very pinched, withered kernels, which contain less than half the flour found in normal kernels. This imperfection is occasioned, in his opinion, either by the meager nourishment that the heads receive when the wheat is turning and when the stem, more or less bent and suffering from a sort of strangulation, does not communicate enough material to the grain, which ripens without filling; or by the

great heat that follows after the wheat has been provided with moisture, which dries the straw promptly and kills the grain before the farinaceous sap has been abundantly transported to it. The experiment I performed by growing a stool in very moist soil exposed fully to the sun, the great number of shriveled grains I found in the heads that developed, prove the correctness of M. Duhamel in regarding this imperfection of the grain as a consequence of heat and excessive moisture.

The two different causes of shriveled wheat that I am reporting are, according to this learned Academician, not the only ones to which he attributes this malady; he assures us that insects, too, sometimes occasion it. I would have wished, and it will be realized at the end of this memoir what the motive of it was. I would have, I say, wished that M. Duhamel had entered into some detail on the nature of the insects that prey upon wheat and that he had indicated the manner of his examining carefully the wheat that was studied for the real causes of the trouble. But he is contented with speaking of the insects without depicting and without really explaining how they materially damage wheat: the following is what he says.

"It happens frequently in cold countries that there are some insects that puncture the stems of wheat before the grain is well filled with that milky substance that must form the flour. These insects deposit their eggs in the epidermis of the straw; and when these eggs are hatched the emerging insects feed on the parenchyma and destroy a part of the vessels; that part from which the head is derived; and, consequently, the grain, not receiving all its material, remains meagre and light, in a word, *shriveled*. It is recognized that the wheat is attacked by insects from the black spots on the straw; for it is believed that these spots are their excreta." Cult. des Terres. Page 220.

ARTICLE II

Description of the principal diseases of Wheat

The different diseases of wheat presented in the foregoing are not, as one may see, those that should be the most dreaded; since, if one excepts the rare effects of a hard freeze that may destroy absolutely everything, the disease consists only in the meagerness of the heads and the diminution of farinaceous substance: real injury, in truth, but

much less than that where the heads are totally blasted and almost never filled with sound kernels. The diseases that result in the entire loss of the head are of three sorts: in considering them, let us follow the order of time in which they appear. The first, less known but as serious as the other two, is that in which one notes a considerable derangement of the stem, the leaves, the head, and in the kind of grain with which the head is filled, before the wheat plant blooms and while the stems, still tender, are not more than a foot or a foot and a half high. The stems of wheat plants of this sort, which I shall distinguish from the others by designating them as *aborted* plants, are ordinarily shorter than stems of the same age; they are twisted, gnarled, and rachitic. Their leaves are commonly of a bluish green color, curled up in different directions; sometimes turned in a double manner, sometimes showing a slight spiral sinuosity, or presenting quite distinctly the figure of a worm-screw.

Aborted Wheat Plants

Although the loss of grain may always be the result of this disease, the circumstances that accompany it are, however, not always the same; the alterations, be they of the stem or of the leaves or of the head, are unequally marked and I have seen such aborted heads whose stems were straight and whose leaves were only slightly curled. The head, in entirely aborted wheat plants and in which the disease is at its height, retains only very little of its normal aspect. It is pinched, dried, and shows only the very imperfect beginnings of as many little glumes that must envelop the grain as of the grain destined to form there. In wheat plants, where the blighting is externally less manifest, the stem is quite straight, the head is formed, the leaves are slightly twisted, the glumes, although shorter than those of healthy wheat, remain entire; but, instead of containing a little white embryo, velveted at its summit, as it is toward the time of flowering, they cover merely a greenish kernel terminated abruptly by a point, and quite similar to a little pea that begins to form in the pod. These green kernels often have two well-marked points, sometimes they have three of them and are configured in a manner seeming to indicate that there may be two or three kernels that, at first, were separate and later may have been united by starting from the same base and by growing within the same glumes.

Without doubt, it may be recalled at this moment that Mr. Wolf regards the abnormality of wheat kernels as a source of disease, and one is tempted to believe that he knew the kind of disease I am about to describe. I, myself, have been of this persuasion, especially in consulting M. Manget, who has commented on the contribution of Mr. Wolf, and who seems to desire to characterize our blighted kernels, by designating them, according to the Act of Leipsic, *monstra tricorporea*. But one will not be slow to recognize that Mr. Wolf, in speaking of abnormal kernels, did not mean those of *blighted* wheat plants, and that if I, myself, have been in error on this point, it was solely because I had not noticed a mistake that had escaped Mr. Wolf. I return now to that that concerns the first of the major diseases of wheat.

Bibliot. Scrip. Med. Tom. 4. Page 644.

When I stated at the beginning of this article that the disease of blighted wheat plants is as serious as the other two diseases, which soon will be before us for consideration, I well realized that certain farmers were not at all in accord with me; in fact, I know by experience that some of them do not know the former disease. If, in glancing at a pile of wheat, they perceive some blasted black kernels, they confuse these with the seeds of cockle, a plant that grows among the wheat plants, whose seeds are black and of nearly the same shape as the smut balls. They do not realize that these bad kernels of wheat constitute a very real, sometimes a very considerable, crop loss rather than a slight increase, as they imagine, in taking them for seeds of cockle. If they are troubled with two diseases, in which the kernels are reduced to a black dust, and especially with the one where the seeds, although ruined, retain their outer envelope, it is less so because of the resultant diminished harvest yield than because of the black dust that comes out of the diseased kernels when the bundles are thrashed and sticks tenaciously to the sound seed, contaminating especially the brush end of each kernel and leaving there a black blemish, quite aptly described by the term *smudge*. This blackening is in reality only a purely external disadvantage, if one considers the kernel as alone designed to serve as food, and one need have no fear of its exerting a bad effect on the health. But the smut-laden kernels are not at all agreeable to the eye, the bread made from them is not perfectly white: also, this

grain is subject to a dockage of one-fifth or even a fourth of the current price when it is offered for sale.

Smutted Wheat Plants.

The second of the major diseases of wheat is of such nature as to command notice the moment the ruined head is free from the sheath; it is not nearly so easy to recognize while the head is still hidden within the sheath. It consists in the almost entire destruction of the head, of which there remains only a kind of core, along the length of which the kernels were attached, which now consist of nothing more than black, dry dust, and some white threads, sole remnants of the envelopes that surrounded the kernels; these remains are commonly the awns that are found at the tips of the glumes and that are hardy enough to resist disease. In these heads thus destroyed, and that I shall call *smutted* in order to give one an appreciable idea; in these heads, still within the sheath, one finds some kernels covered with a white epidermis, very thin, transparent, and only meagerly developed. These kernels are black within; the scant envelope that covers them and adheres within the sheath, probably dries on being exposed to the air; the entire head, emerging at once, hardly differs from those heads that we have said consisted only of a dust and of awns, remanants of the tips of the glumes.

Bunted wheat.

The third disease of wheat, which constitutes the principal thesis of this memoir, is that in which the kernel, retaining almost its normal form and its own bulk, is converted into a greasy and blackish powder, and becomes easily detached from the base of the glume when it is very dry: I shall designate it in due course under the name of *bunted* wheat. One begins to distinguish, before the completion of the flowering period, the most advanced heads among those attacked by this disease. As long as the heads are enclosed within their sheaths, even in full daylight, one suspects nothing wrong with the plant; the stem is straight and upright, the leaves are commonly without defect; but when the wheat plants blossom, the bunted heads are recognized by their bluish color; the glumes, which envelop the kernel, are more or less spotted by little white patches; the kernel itself, larger than it ought to be naturally, is very dark green; so long as it retains this color, it adheres to the base of the glume like a sound kernel; its three stamens, much less extended than it [the bunted kernel] and adherent

to its sides, drooping and withered: if one opens this bunted kernel, one finds it filled with a greasy, blackish material, from which a fetid odor is given off, especially when one crushes the bunted kernel between the fingers.

Under the name of *smutted wheat* M. Duhamel apparently includes the second and third disease;[2] although both seem to have the same origin, I have felt that we should separate them. They offer, in fact, some quite noticeable differences. The second develops more suddenly than the third; the glumes of the *bunt ball*, even to its epidermis, are very little altered, while in the *smutted* plants they are entirely destroyed. The dust particles of the *bunt balls*, viewed under the microscope, are larger than those of the *smutted* kernels: In a word, nothing remains of the heads of plants affected with the latter disease, neither well-formed kernels nor pistils nor vestiges of stamens. That is to say, everything is consumed and reduced to a dry dust, which the least agi-

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[2] This memoir was not yet before me when I received M. Duhamel's contribution entitled *Series of Experiments, etc.* 1752. In the 3rd chapter of this paper, the author speaks of *black wheat, blighted or smutted;* he made the distinction between the two diseases by the black dust into which the substance of the kernel is changed.

By these words *Nielle proprement dite,* he means the disease that I have designated by the name *smutted wheat;* and by the *malformation or smut,* that which I have indicated under the name of *carious or bunted wheat.* I was surprised to find in this publication no observation on the peculiar condition I have included in the number of the principal diseases of wheat under the name of *aborted wheat.* Perhaps it is only rarely observed in the cantons that have come under M. Duhamel's observation; or perhaps the aborted kernels have been confused with the carious ones in assuming, like some growers of this country, that there are years when the kernels of diseased wheat crush under the fingers and become reduced to a black dust, and other years when they simply dry out and yet retain a certain consistency.

In the description I have given for the blight disease it is thought well to distinguish it from the other two diseases of which M. Duhamel speaks. Moreover, I am able to state that it is very common in the different cantons I have inspected. I carefully retained several of last-year's wheat plants that were attacked by this kind of disease and that have been subject to my local observations, without counting those I have made at large on a large number of other plants affected by the same disease.

The work of M. Duhamel, now under consideration, has not failed to interest the public. Always due consideration is given to new principles of soil management. It is learned with pleasure that in different countries enlightened men, zealous citizens, confirm by proper tests the soundness of those principles and recall the best days of Rome in vouchsafing their devotion to agriculture without any loss of personal worth.

Aside from the interest I take, as a citizen, in this entire work, I, as author, call special attention to its third chapter. It gives me opportunity to do justice to M. Duhamel whose elucidations have been very helpful to me. I have noted there that the observations of this celebrated Academician square very well with mine without causing one to suspect that his last contribution, which did not reach me until after I had sent him this memoir, had served to guide me and put me on the way. I note in this same chapter that M. Duhamel considers, as I do, that nearly all the causes to which the principal diseases of wheat have thus far been attributed have been advanced without fundamental basis; and I have noticed there, unless I am mistaken, several things that, well developed, agree with the opinion that repeated experiments will lead me finally to adopt.

tation precipitates. In the bunt balls one perceives no noticeable derangement; the glumes adhere; the kernels, although inwardly reduced to dust, retain almost their normal shape: one still sees the velvet-down of their tips, the pistils and stamens remain: the disease in the *bunted* heads is almost wholly internal. In the *smutted* heads, it seems as though there had been a fire; the loss is complete.

It is recognized that we must distinguish the two different diseases of wheat, and, in naming them, we must not employ the too general names of blast, blemish, frost, etc. It is still more necessary to realize that the blighted kernels, in particular, do not contain any dust like that of *bunt balls* and like the *smutted kernels.* The blighted kernels are as green as the culms that bear them and are filled with a glairy substance; they dry at the same time the stems do, shrink, and become black, but retain their shape, nevertheless, although withered and void of sap.

It is not necessary, however, to regard these three separate diseases of wheat as so distinctly unlike that they may never be confused, although the symptoms of the one are never those of the other, and although they may always be designated by all the indices that we have said accompany them. Among the great number of diseased wheat plants on which I have made observations, I have sometimes found some *bunt balls* in certain heads affected also with *smut;* some blighted kernels in some heads of which the majority of the kernels were *bunted;* some straight stems that bore blighted kernels; some twisted leaves on *bunted* wheat plants: I will say even more; I have encountered some heads laden with sound kernels, although borne by tillers whose leaves were curled, and indicative of blight. But these cases are all rare; the differences presented by the principal diseases of wheat generally are such as we have established: if there are some exceptions to be noted, one should disregard them and consider as constant in this article only that that most commonly occurs. I shall add here an observation that we have not had too clearly in mind, because it relates obscurely to the three kinds of diseased kernels I have differentiated. That is to say, these spoiled kernels are sometimes found associated in the same head with one or several normal kernels; or develop on tillers

belonging to a root system from which other tillers may spring, laden with sound grain.

It is opportune now to examine the opinion of Mr. Wolf on the cause of the disease of grain: I realize in advance that this discussion will tire the reader; but it is desirable to give attention to the opinion of a distinguished scholar whose high reputation has already had some influence on this subject.[3] One owes to great men, when one dares to take issue with them, the right to develop their ideas and to make easy to the public the means of presenting them.

Mr. Wolf, without distinguishing between smutted and bunted wheat plants, contents himself with saying, "that it often happens that the kernels of a head of wheat become black as coals, and soften finally more and more until at last they dissolve into a black dust, fine as flour." A con- § 1
dition to which he gives a general name, such as that of *smut*, in our language. After these few words that relate to wheat, the subject, as presented in the chapter by Mr. Wolf, is one of no further research interest to him. He cites some experiments, but they are made on barley and oats: his special tests are applicable to wheat, only, in so far as what he relates concerning the seed of barley and oats may, according to him, apply to wheat, and also, as he believes, the diseases of these three crop plants have one and the same origin. Let us enter now into the matter of finding out what Mr. Wolf observed in barley.

"When in 1716," says he, "I tried to determine by ex- § 14
periment the cause that I had assigned to the multiplication of wheat plants, chance favored me to the extent that a single kernel of barley, planted in a box that I kept in my bedroom during the night and exposed during the day at an open window, without rain or artificial watering, produced some healthy heads and some that were smutty. . . . When the smutty heads formed I observed at first a considerable difference in the beards; for, instead of the beards of a head of barley being long, straight, and green, like those of a healthy head, those of the smutty head were much shorter, curled, twisted, and whitish. The following I perceived early in the smutted heads.

[3] In the *Oeconomic Journal* of September, 1751, page 13, and following, there is a memoir on smut, in which the opinion of Mr. Wolf is accepted. The author of this memoir regards as constant the abnormality of kernels of wheat reduced to black dust.

§ 17 The smutted grains were more exposed than the sound ones, for the most part black, and only here and there spotted with green. Their shape differed noticeably from that of the good kernels; for, toward the distal end, the kernel was divided into three parts, as if there had been two other kernels ready to grow; or as if three kernels were joined together in a manner to form one; with the help of a magnifying glass, moreover, one could see clearly that a
§ 19 grain of smutted barley was a monstrosity made up of three bodies. As all the kernels of my smutted head were of the same form, I walked through the fields where I found several heads of smutted barley the grains of which were all similar
§ 20 to those that I had at home. . . . Thereupon, I recognized clearly that the smutted kernels were nothing other than monstrosities: and, since liquid materials spoil when they
§ 21 stagnate, I judged that the sap, which circulates in all plants, had stopped in its movement, because of the abnormal shape of the kernels, and spoiled because of stagnation. . . . How does it happen that nature produces abnormalities among wheat plants? This I make no pretense of determining here. . . . However, it is seen that up to the present there is no remedy against smut, and, that, as a consequence, it will be necessary to continue to perform experiments in order to discover the causes that produce an abnormality in nature."

In order to present more clearly the opinion of Mr. Wolf and be able to speak from my own observations, I examined carefully several smutted heads of barley held over from last year. These were heads of common barley, generally designated *Hordeum distichum*, and not of another species known under the name of *Hordeum polystichum*, the heads of which are of an unusual shape.

The diseased barley grains that I have observed have quite the form ascribed to them by Mr. Wolf, that is to say, three grains united to form one. The envelope that covers these smutty grains is nothing other than the glume itself; in fact, it is known that the envelope proper of the barley grain is extremely delicate, and that the glumes, which one can not too much admire, adhere to the grain itself, thus making up for the delicacy of the true envelope. This sort of artificial envelope of the barley grain, comprising the glumes, is the only one left on the smutty grains. It is not,

however, so well preserved as that of the bunted wheat kernel. One rarely finds grains of diseased barley that are not ruptured at some point and that thus expose to view the black dust with which they are filled.

In carefully examining the heads of barley above referred to and comparing them with heads of healthy barley, also held over from last year, I have pointed out that in the sound heads the well-ripened grain is joined at the base by two false grains, which always abort, and at the expense of which the middle grain is nourished. From that moment I have been of the opinion that the malformation caused by the smut might bring about the almost complete fusion of the three grains. Mr. Wolf observed that all smutty grains of barley have the same configuration, that they present three apices, and are like three grains joined to form a single one. It would be very unusual if a malformation, an imperfection in nature, varied in no respect in different plants, in barley of every country, if this deformity had not everywhere the same cause. I wish to speak here of the existence of three bodies that are easily distinguished in their normal state but are found united through some accident and are unable to develop as individual grains.

I come now to the very foundation of Mr. Wolf's theory. He considers the black dust of the diseased grains to be the result of a defect in the circulation of the sap within the malformed body. This opinion seems specious if in the light of it we care to consider smutted barley grains as so many malformations. It would appear quite plausible in relation to aborted wheat grains, which may be considered even more malformed than smutted grains of barley. But will this opinion hold when applied to the bunted grains of wheat? These latter are not to be regarded as monstrosities. On drying, they maintain very nearly their natural shape. Internally, however, they are reduced to black dust without any outward indication that the movement of the sap had been checked there; without one's justifiably suspecting that the sap, in thus stagnating in the bunted kernels had there become decadent material, for the cessation of the movement of the sap is attributed solely to the deformity of the grain.

Let us conclude with the detail here recorded.

(1). That Mr. Wolf, in speaking of malformed grains of

wheat, did not mean those to which I have applied the term *aborted*. The 3-body abnormalities of which he speaks are grains reduced to black dust; they belong to the order of smut grains, whilst the grains that I have described and have observed only in wheat contain no dust and must be separately classified.

(2). That Mr. Wolf meant by malformed grains of wheat neither bunted grains, since their shape is well maintained, nor smutted grains, since it is very seldom one finds them intact in the head, except in the early stage of the disease, a stage very difficult to obtain, but one in which the grains possess their characteristic form and differ from the sound kernels only in some black spots with which they are blemished.

(3). Finally, that the barley disease is the only one Mr. Wolf has carefully considered. That he was struck by the abnormal shape of the diseased grains of this plant; that he believed he saw in their malformation an evidence of disease; that, without examining the shape of the wheat grains that had been converted into dust and those of oats attacked by the same disease, he applied to these two plants the same theory he had applied to barley.

ARTICLE III

On the common disease of Rye known as ERGOT OR SPURRED WHEAT.

Before closing this chapter I believe I should recall here that singular disease of rye known under the name of *spurred wheat* or *ergot,* because, in fact, the grain of rye thus attacked has somewhat the appearance of a cock's spur. This disease bears some relation to my subject: I have noticed it with surprise on wheat, and without doubt it is due to one and the same cause in the several plants we shall find subject to it.

Languis, physician and learned naturalist, is one of the contributors who has the more carefully examined the disease of rye and who has gone into the greatest detail either in describing the deplorable effects, which, according to him, follow ergot, when he finds it mixed abundantly with bread, or in depicting with exactness this defective grain and in explaining the cause to which he believes it is to be attributed. In fear that I may wander too far from

my objective, I shall not cite the observations made by him as a physician. One cannot read them, however, without becoming keenly interested in them from the standpoint of humanity. I shall, therefore, content myself with relating summarily the place where he recorded the description of ergot and where he tried to explain the cause of it[4] *Clavi Etc.*

I do not know whether the distinction made by Languis between one kind of ergot, very baleful in itself, and another that is not at all so, is based on experiment. I would willingly believe that this kind of diseased seed always is of bad quality, but it is noticeably injurious only when a certain quantity is mixed with the rye flour. This idea is supported by consideration of such years as 1709, when the sad effects of ergot were especially pronounced. It was then more abundant than usual, since, according to the report of M. Noël, Surgeon of the Hospital of Orleans, who transmitted some observations on the subject to the Academy of Sciences, nearly one-fourth of the rye of the Sologne consisted of ergot grains, which the poor people neglected to separate from the good grain because of the extreme scarcity following the long winter. We shall now return to Languis who explains the cause of ergot.[5] *Excedenti vero aeris* Etc.

Hist. de l'Acad. des Sciences 1710. Page 63.

I shall not stop to raise objections to the opinions of Languis, who attributes ergot to excessive atmospheric

Description merborum ex e su clavorum secalinorum cum pane. Lucernae 1717. Act. Lips. 1718. Page 309.

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[4] *Clavi (l'Ergot) Sunt morbus granorum secalinorum qui in eorum excrescentiâ. magnitudine auctâ & formâ coloreque depravato consistit. Sunt hi clavi, accuminatâ suâ parte vaginæ implantati, instar reliquorum granorum, quam, propter crassitiem suam, nimis distendunt; coloris fusci, nigricantis, duri, modò teneriores, modò crassiores, recti, vel incurvati, aut cornuti, saporis frumentacei, cum acrimontâ tamen conjuncti; juxtà longitudinem binas, ternas, vel plures nanciscuntur fissuras . . . Dum conteruntur non farinam volatilem concedunt, sed pulverem graviorem . . . Accensi clavi facilè flammam concipiunt, sed obscuram & nigriusculam & cinerem post se relinquunt nigricantem, splendentem tamen instar cornuum vel crinium combustorum. Non omnes clavi infaustos edunt effectus, verum venenati tantum; quotannis enim tales in spicis frumenti concipiuntur; omni tamen venenositate carentes Venenosi in egenis potissimùm deleteriam suam exerunt vim ut potè qui nonnisi secalino vescentes pane nullam in clavis à bonis granis separandis aut frumento debitè exsiccando adhibent curam.*

[5] *Exedenti verò aeris humiditati adscribendus est nimius clavorum proventus, cui ros, nebula, pluvia socias adjugunt manus. Aer enim humidus, ratione subtilium & levium suarum particularum, fortiter penetrat, nitro, sulphure volatili aliisque subtilibus particulis imprægnatus. Hinc spicam ambit vaginam emollit, extendit, premit involucrum farinaceum ad putredinem disponit, cum pericarpio se uniens in fermentationis motum rapitur, sicque augmentum grani promovet . . . Venenositas (granis) accedit à rubigine, ut potè quæ ao potissimum tempore descendere observatur quo grana frumenti in clavos excrescere incipiunt; sub formâ etiam materiæ cujusdam viscidæ dulcis eis adhærere inventa fuit, ac tandem iisdem gaudet facultatibus, quibus clavos nocere diximus, visciditate putà & acrimoniâ . . . Rubigo ex aequæis, insalubribus tamen ac venenatis constat particulis quæ de die ex terra & aquis impuris sublatæ vel per ventos adductæ, noctuque condensatæ manè delabuntur . . . Gaudet hæc rubigo visciditate dulci cum corrosivâ tamen acrimoniâ conjunctâ.*

humidity, dews, mists, and rain; who claims that very moist air laden with nitrous, sulphurous, and other volatile components, impinging upon the epidermis of the grain, softens it, renders it liable to contamination, even to marked penetration of the grain, excites within it a fermentation, swells and extends it excessively; and who claims that the harmful quality of certain ergotized wheat depends on a viscous, acrid, corrosive substance, very noticeable in plants and occurring especially at a time when ergot begins to form. The reflections that I make at this moment on the opinion of the celebrated physician, who attributes ergot to the malignity of certain mists will apply naturally to those of Languis.

The Historian of the Royal Academy of Sciences says, in considering the explanation given by M. Fagon for the origin of ergot (b), that "there are mists that injure wheat and against which the majority of rye heads are protected by their awns. In those that this malign humidity can reach and penetrate, it disintegrates the pericarp, which covers the grain, and blackens and alters the substance of the grain itself. The sap, no longer confined within ordinary limits by the pericarp, is carried into the grain in greater abundance and, collecting irregularly, forms a sort of malformation, which, moreover, is harmful because it is composed of a mixture of this superfluous sap with impure moisture. It is only in rye that one finds ergot. . . . This ill-appointed kind of grain, it is stated farther on, occurs, according to a naturalist who had consulted M. Delahire, in great abundance on damp, cold soil and in rainy years. A certain variety of rye, which is sown in March, is more subject than those varieties that are autumn-sown.

Hist. de l'Acad. des Sciences 1710. Page 63.

I feel that M. Fagon has some weight of authority, but I can hardly understand how the mists, said to be the cause of ergot, fail to produce this singular malady in barley, oats, etc. If one considers the awns of the head as being able to prevent the mists from penetrating the grain itself, this explanation no longer stands in face of a prodigious quantity of heads of wheat that are awnless and in which one almost never finds ergot. Moreover, one might call attention to the fact that the mists ordinarily cover a certain extent of ground and, being supposedly injurious, ought to produce a quite general effect. Experience does not here square with

theory; a given head is ergotized, its neighbor is not; you run through several rows where the heads are healthy, suddenly you see two or three with ergot; sometimes an *arpent* shows ergotized wheat at every step, while that of another lot alongside shows none whatever, or but very little. An affected head of rye, on the other hand, bears one, two, three, up to eight ergots, according to M. Dodart: these cornute grains are more or less bulky, more or less elongate. I have kept several of them intact in the head, one among others of which the shape approximates that of a curved spindle; it is one-and-a-half inches long, over and above its curvature, and about two lines in diameter at its thickest point; but I have never found a head of rye in which all of the kernels were ergotized; and I have never read a memoir on this subject in which mention of an entirely ergotized head was made.

Mém. de l'Acad. des Sciences. Tom. 10, p. 562.

Without doubt, it would be vain for me to point out the fact that one is in error in attributing to humidity the genesis of ergot, as this disease attacks rye sown in elevated situations as well as that in the lowest situations, and during dry years as well as rainy ones. I shall merely note that M. Fagon deceives himself in asserting that ergot is a malady peculiar to rye: I have observed this ergot and have frequently seen it on a little plant known among botanists under the name of *Gramen loliaceum*, on *Gramen micosuros*, and on true darnel grass. Here are the facts on the subject of these three plants that should be brought out; facts that banish every notion attributing ergot to humidity.

First Fact. One of my friends[6] has shown me some darnel grass that gave no indication of ergot, although it came from a place that had been inundated by spring water, and where this unfortunate condition had brought about the complete failure of rye that had been sown there. This friend, exact observer that he was, assured me that there was in this same inundated area a considerable quantity of darnel grass; that it was very vigorous, judged from the

[6] This friend, whom to-day I am free to name, is M. Ludot, known among men of letters through a prize obtained by him in the Royal Academy of Sciences in recognition of rare talents and a modesty even yet more rare than his talents. I am indebted to him for numerous nice observations on the subject under consideration. He has communicated them to me with that disinterestedness, and I dare say, that simplicity that characterize a gracious spirit and that nearly always mark the superiority of the enlightened.

number of culms as well as from the length of heads and abundance of seed; that he had examined it carefully but had, however, perceived no ergot.

SECOND FACT. In a place, also inundated by chance springs, where some wheat had been drowned beyond recovery, there were found four or five tufts of *Gramen micosuros*, comprising 70 to 80 culms each. Ergot appeared in only one of these tufts; yet there were but 12 or 15 culms of this one tuft the heads of which bore one or two ergotized grains.

THIRD FACT. Ergotized *Gramen loliaceum* is found from August to October, inclusive; it develops everywhere, indifferently during dry as well as wet seasons; it grows in the middle of a field, on a high place, on the borders of a little ditch, in a trench; I have noticed it in abundance on the bulwarks where it was certainly well assured against moisture.

In declining to accept the explanation M. Fagon has given for the development of ergot, I hesitate only to consider mists as the primary cause of injury and I accept without reservation what he says about the formation of ergot itself.

In fact, I believe that in assuming the cause to be due to the sting of some insect, rather than injurious humidity, one may state with some probability and ovbiate all difficulties that the pericarp of the seed becomes half-opened following this puncture; that the sap there present, being no longer confined within ordinary limits by the pericarp is present in great abundance and, accumulating irregularly, produces a sort of malformation. Let us add also that this insect may leave in the grain it has punctured a subtle and vinous liquid that ferments in the farinaceous juice tainted by it, and may serve to explain why ergot copiously mixed in bread becomes a kind of poison and produces in those who have eaten it scorbutic troubles, and sometimes occasions gangrene: while the diseases of wheat, barley, and oats never produce any subsequent bad results.

Let us recall here a point in the description of ergot given by Languis: *Juxta longitudinem (clavorum) binas, ternas, vel plures nansiscuntur fissuras*. The fissures that this author has noticed along ergot grains may, without doubt, result from desiccation subsequent to excessive swell-

ing. But would it be extraordinary if these openings had their origin in the pericarp puncture produced by an insect? Would it be surprising if they took the form of longitudinal fissures, since their edges are pushed apart by means of an excess of farinaceous material there present and, since these cracks in time are evident after a part of this material has become dry and shrunken and the ergotized full-grown grain has, by this time, become wrinkled, black, and completely withered?

In attributing ergot to the sting of an insect I have ignored no opinions received. M. Marchant has given a description of a kind of tuberosity or extraordinary growth he observed on a radish and that he believed was caused by insect puncture. One should consider also as the result of a sting those swellings or galls that one sometimes observes on *Sinapi arvensa minus*, whether on the stem or on the branches or in the region of the flowers. The excellent memoir of M. de Reaumur on the galls of trees, shrubs, and herbaceous plants; the surprising details into which he has entered relative to the different insects whose stings engender galls, should familiarize us with these singular but purely natural results and show us their similarity to what I, perhaps the first, have advanced on the cause of cornute wheat. It will undoubtedly appear to have been proper to have reported at the beginning of this digression the substance of a memoir of M. Dodart on cornute wheat, one of the first contributions made to our knowledge of this sort of grain disease. But, in speaking of ergot, I have had in view principally the authors who have, themselves, tried to explain the cause of it. The description of cornute wheat given by M. Dodart is much briefer than that by Languis. If he attributes ergot to excessive heat followed by moisture, it is only after several letters written to him, which vary, moreover, among themselves, that an attempt is made to detail the evil effects of ergot. It is of this contribution that M. Dodart has made the abstract. I have observed there a statement that supports my view, contrary to the distinction made by Languis between injurious cornute wheat and other wheat not thus affected. There is scarcely a year, he says in the abstract, when a few of these grains do not develop; when there are but few, one does not notice the bad effect.

Mém. de l'Acad. des Sciences. 1709, p. 64.

Hist. des Ins. Tom. 3. Mém. 12.

Mém. de l'Acad. des Sciences. 10. page 562.

La Mérid. de Paris, suite des Mém. de l'Acad. des Sciences. 1740. 3me partie page cxiv.

I shall close these elucidations on cornute wheat by citing a passage from the natural-history observations made in one of the provinces of central France, by M. Lemonnier. The authority of this scholarly academician fortunately threw me into perplexity on a point that I had always regarded as certain. Following is the passage, faithfully excerpted: "The crops (in the southern part of Berri) are often damaged by hot, humid winds, which prevail during the months of June and July, and one sees then much blasted or ergotized wheat."

Wheat, therefore, also is subject to ergot. I at first suspected, on reading this observation by M. Lemonnier, that he would be able to avoid giving to the kind of disease he had observed in wheat the name ascribed to the disease of rye. I know that all those who have written on the untoward circumstances to which wheat is exposed had never included ergot, and I was then able to assert that the great number of heads of wheat that had passed through my hands none had contained any ergots. But I am now certain that a grain of wheat can become ergotized. The case is, however, extremely rare; I have yet noted it but twice in different parcels of wheat situated in the neighborhood of Troyes.

CHAPTER IV

Special observations on bunted grains of wheat. Conjectures on the cause of this trouble

I HAVE intimated in the foregoing that the sting of some insect might be the cause of the rye disease. I made the statement on the bold presumption that I might thereby solve several difficulties, and find support in examples that very carefully examined plants quite frequently furnish. If, without daring to assert that insects do injure wheat, I suggest farther on some convincing opinions, it will no longer be on a mere presumption solely with a view to accounting for everything and satisfying myself with some recognized effects. I shall cite some exact observations; I shall indicate the little creatures that one might suspect of being the originators of the trouble; and I shall enter into details that, even though they may have no direct bearing on our objective, do bear at least some relation to it and perhaps may sometime be found pertinent. It is well known that I have kept an accurate journal of every interesting thing I have found in my examination of wheat. But this record contains some data that have not been at all sufficiently verified and others that have been noted several times: I shall therefore be content to give the substance, hazarding nothing, and present simply what I have observed.

Convinced as I was last spring that the disease of wheat should appear early and attack the still tender stems, I went through several plots of wheat in which the culms had begun to form. Up to the time when they were only seven to eight inches high I noticed no derangement; but when they were about a foot high, the first thing to strike me was the inverse rolling of the leaves; I examined them carefully and noted the fact that they frequently belonged to twisted as well as knotted stems. At time of emergence of the heads, I was particularly concerned with observing these malformed stems; I carefully examined their heads and at once removed the glumes in order to examine the enclosed kernels.

Instead of finding a white embryo, velvety above, such as a sound kernel shows in its incipiency, I generally found only some small green grains terminated by one, two, or three points, often exceeding in number what a single head should produce, sometimes joined together in a sort of malformation, such as we have already spoken of.

I at first believed that this disease of wheat was the one in which I was particularly interested and that these little green grains, once having attained a certain size, would become converted to dust and be nothing other than carious kernels. With this idea, which I held for some time, because the moment for recognition of wheat affected by this last kind of disease had not yet arrived, I marked several malformed stems so as to distinguish them on occasion and to follow in them the progress of the disease. But, the more I advanced in my observations, the less did the little green grains appear to change into bunt grains. Instead of swelling and assuming shape, they became a bit desiccated, their envelope turned brown, and their glistening substance became reduced to nothing.

In early June, at a moment when I was without anything distinct to observe, I forced open the sheath of a head, more backward than the others and borne on a malformed stem. I saw a swarm of tiny black insects come out, creatures alone rendered noticeable by their color. They spread out over the culm, crawled about for a moment, and then reentered the sheath they had left and that had automatically closed. I devoted myself from that moment to a consideration of all culms thus harshly dealt with; I examined them carefully, and I commonly found there insects similar to those first observed, I collected and carried to my home a little sheaf of these malformed stems, bearing inversely curved leaves, in the sheaths of which numerous insects remained enveloped.

These little creatures belonged to the *Staphilinidae*, insects that, like the earwigs, have very short wing covers under which their wings, large enough to support them in the air, and like transparent gauze, are admirably folded. They have two antennae formed of several segments placed end to end like the beads of a chaplet, the last segment terminating in a point; some hairs are noticeable at the point of juncture of the first segments to the side of the insect's

head. These basal segments are of a darker color than the others. I stated that the antennae are composed of several segments because their number is not fixed for each insect; that depends, according to every appearance, on the age of the insect. The antennae of the youngest consist of but two or three segments. The older ones commonly have up to six. I have seen some insects in which one antenna was longer by a segment than the other, due, no doubt, to some accident. These insects have six legs of unequal size, each composed of two articulations; their bodies, from the extremity of the corselet to the tail, appear to me to comprise nine rings; the tail ends in a point and bears at the tip two hairs disposed in the form of horns.

Assuming that these insects might cause considerable damage to wheat, being commonly found on wheat in the different cantons in great numbers and indistinguishably, I examined these little animals carefully; I continued to open the sheaths of the heads in which I suspected some damage; I pushed aside the glumes which enveloped the grains; I often saw there some insects, but I did not make out their behavior nor what part of the wheat plant they had altered: I clearly perceived that they were ordinarily more numerous and most abundant on the malformed, bluish culms bearing inversely rolled leaves; but I never chanced upon the initial moment of their attack; the injury, if they really produced it, was done at other times than those of my observations.

About the time when I first discovered the insects, that is to say, before the wheat blossomed, I noticed, and this observation was the same in the different cantons, I noticed, I say, several heads entirely free from the boot, ruined and reduced to a black dust; I found others similarly ruined, althought well enclosed within their sheaths and showing no marked indications of deterioration. These heads, to which we have given the name *smutted* heads, only rarely showed any insects; it seemed to me that it [the smut] was more often found on enclosed than on exposed heads.

When the plants finally blossomed, several heads were observed in which the glumes and grains were dark green. I opened these grains; they were filled with a greasy material, blackish and foetid. I recognized these heads to be especially those I was seeking; in examining them carefully, I acquired the facility to recognize them at a glance; I col-

lected a certain number of them and took them to my home where I could examine them at leisure. One of the first things about them that impressed me was the discovery of some red insects mixed with the black ones, but a little smaller than the latter: I observed them under the microscope; they were wingless; but, with this exception, their configuration and their parts were the same as those of the black ones. I did not doubt but that the red and black insects were of the same species and I confirmed this conclusion by the lack of uniformity of color in the new insects, some being a brighter red than others, and yet others that had little black spots on the back indicative of a metamorphosis and a change from red to black.

The red insects being more common than the black during the flowering period of the wheat, I had every possible facility for their observation, and I found them quite often even on the sound heads. The examination that I made of several glumes covering the bunt grains was very helpful and threw some light on the subject of the insects; it convinced me that these little creatures made their home ordinarily in the heads of wheat and that they laid their eggs there; that from these eggs came worms; and that these worms, after passing the condition of nymphs, became in time the insects I observed. I noticed several times, in fact, and in a manner beyond question, their excreta on the glumes of the bunt grains; I noticed there some yellow shells where the insects were in the nymph stage, while others were white and open at the end whence the insects had emerged. I frequently found on the glumes or on some of the grains little dead insects still clinging to their cast-off remains and others that were scarcely born and with antennae composed of but two segments. It was not on the bunt grains alone that I noticed insects of every color and age, their excreta, their cast-off remains; I found them also, though less frequently, on the sound grains and within their glumes; I sometimes noted, by the excreta remaining on the velvety hairs of the grain, that some of the insects had crept over a sound grain.

These observations indicated something, to be sure; but they taught me nothing concerning the damage the insects might work within the head and the kind of nourishment they derived there. In the hope of being better informed, I

decided one day to put under a microscope a sound incipient kernel the flower of which had but recently passed. The two pistils and the top of this kernel were still laden with dust from the stamens. I placed on this grain a red insect and observed it attentively. This insect made at first several rounds over the surface of the grain, finally it slipped between the hairs of the tuft at the top, crept amongst them a long time, and, in spite of the difficulty experienced because of the length of its antennae, suspended itself by one leg to a filament of one of the pistils, returned to the grain itself, reentered the tuft, plunged its head among the hairs by bringing its antennae together and resting motionless in this situation, without making other movement than that of the tail, which it moved only at intervals. After having left it at rest nearly a quarter of an hour, I agitated slightly the grain on which it lay; it did not move, at all, and I had to touch it with the point of a pen in order to make it take leave.

I repeated the experiment several times by placing insects on the grains; they crept willingly, mingled straightway with the hairs of the tufted top, plunged their heads among them, leaving them momentarily, then returning, as if to a place they had found agreeable, and rubbing their legs against each other in order to remove the dust of the stamens that had become attached there.

After several observations made on insects that I had by chance found on some of the grains or had expressly placed there, I have not perceived that they penetrate some spot and obtain nourishment from either the grain itself or from the pollen scattered over the tufted top. It was observed, however, that a red insect had thrust its head between the hairs at the distal end of the grain and that it remained there firmly attached. Might one not presume that it sucked therefrom some liquid with which the base of the hairs was moistened? Does not this liquid come from the particles of the fecundating dust, which, dilated by heat, swell and open and thus permit their oily juice to run along the hairs, thereby serving to moisten the tip of the grain? I scraped onto a small concave glass a certain quantity of these fecundating dust particles. They yielded several droplets of a very clear liquid. I exposed to the sun some of these very dust particles under a small receptacle; the top of

Hist. des Ins. T. 5. p. 414.

the receptacle became moist as though it had been exposed to the vapor of hot water. This idea of an oily sap coming from the staminate dust resulted from observations made by an able master whom I shall cite in quoting M. de Réaumur. "M. Bernard de Jussieu made a microscopic study of the staminate dust of many kinds of cruciferous flowers, such as those of mustard, rocket, etc. He studied, I say, these dusts while they were in the water in which he had placed them. He observed that these little grains swelled more and more until they cracked. At the moment when each grain cracked, there came out of it a jet of liquid, which floated on the water without mixing with it and, which, therefore, must have been an oily liquid. The same experiment was repeated, with the same success, on the dusts of plants of several different classes."

Although one cannot assert too postively that the little tuft of hairs that crowns a grain of wheat is moistened by a juice produced by the staminate dust and that our insects attach themselves there in order that they may be able to procure a more substantial diet, the manner, however, in which they thrust their heads between the pistils and into the hairy tuft of the young grain would lead one to believe that they find there something foreign to the hairs of the grain and very attractive to them.

This concept that the red insects may, possibly, consume the juice produced by the staminate dust and that this diet may meet their needs, led me to believe that the black insects, being of the same species as the red, had the same taste as they, and that, being more vigorous, were able to feed on the nondissolved pollen grains, while the anthers containing them were yet tender and easy to puncture. I hesitated to accept this idea when I considered how the anthers belonging to carious grains are partly open, withered, and of an orange color, although still situated at but half or two-thirds the height of the kernel when, as a matter of fact, they should, normally, open and show indications of maturity only when they extend beyond the kernel and are about to project beyond the glumes. I thenceforth regarded the black insects as the indirect cause of the bunt grains, since these grains, not being themselves actually damaged, and, actually bearing their short stamens, appeared to me to be deranged through failure of fecundation,

a failure to be attributed, I believed, to the opening made in the stamens and to the loss of pollen. I was supported in this by an observation recorded in my journal. To be sure, it is recorded there but once; but without regarding it as confirmed, I made note of it as follows: Page 226.

"The carious grains are invariably green: I noticed that those in which some portion is white had one or two stamens taller than those commonly lateral to the carious grains and were able to manifest a little fecundation, their pistils still showing some pollen grains produced by the stamens projecting above them" [the pistils].

I did not ignore the fact that M. Duhamel, in his treatise on the culture of soils, said that he had "quite sufficiently proved that all the grains that are not fecundated do not become smutted." And I would not have hesitated to accept the authority of this scholarly academician had he spoken from some personally conducted experiment; but he explained in a general way and I did not see that he had looked particularly into what became of the nonfecundated wheat grains. As for me, I certainly had not seen any bunt grain in which the rôle of the stamens had been carried out. I observed no healthy kernel in which the stamens had been functionless.

The statements of M. Duhamel seem to me to require further explanation. The grains of wheat, considerably altered from the start, develop on stunted culms and become desiccated as they grow; these grains, I say, will not become bunted, it is true, or smutted, according to M. Duhamel's statement, even though they have not been fecundated; they will terminate in blight. But some of the wheat grains that have no noticeable defects, that develop on apparently healthy culms, and that grow from the moment of fecundation by receiving an abundance of nourishment; these grains will become diseased, bunted, if, at the moment fixed by nature, they are not penetrated by a juice capable of engendering fermentation within them and thereby developing their contained germ.[1]

[1] Grape seeds, which have not been fecundated are not ruined, it is true, and even reach maturity, remaining much smaller than the fecundated grains: but one should not always draw conclusions about one plant from another; the difference in organization is a source of varieties. One often notices that a given cause does not produce a like effect in different plants. Moreover, the grape fruit should not be compared with a wheat seed; it is the seed, itself, of the grape that should enter into the comparison

Although I realize how difficult it is to determine the actual cause, the primary source of the blight of wheat, I cannot wholly abandon the surmises that the aborted stamens of the bunted grains and the resultant failure of fecundation have raised in my mind. Perhaps some grains of wheat would not become bunted through this failure alone; possibly there is a hidden cause of both the decay and the blighting of the stamens themselves, apart from this latter being the origin of the first. But it is worth noting that there is always failure of fecundation in the bunted grains; at least, one may perhaps believe that the blighting of the stamens, the one conspicuous abnormality of these diseased grains, may be the origin of some imperfection.

One may here clearly see how I infer as solved a question that the authority of a celebrated botanist, it would appear, should render undecided. According to M. Tournefort's theory the pollen grains are merely the superfluous remains of the grain, and the filaments those of excretory canals by which the nascent embryo discharges useless juices by depositing them in the anthers. But the opinion of this eminent man is no longer held. M. Geoffroi, in an excellent memoir on the structure and use of the principal parts of flowers, has presented some new views; he has given to the stamens more worthy functions than M. Tournefort had conceived. He regards them as necessary to the fecundation of plants; he shows that the pollen of the anthers is designed to fall on the pistils, and that nature, being uniform in this particular for different plants, relates them to the common order and ascribes to them a place in the scheme of generation.

Mém. de l'Acad. des Sciences, 1711. Page 207.

To return to our objective, one really cannot knowingly consider a head of wheat, about to bloom, without being convinced that the stamens are essential parts and have been so disposed as to insure their function. The anthers borne on the extremely delicate filaments are arranged around the grain, they extend above it as they mature: When they have attained a certain height they are orange-tinted; their epidermis is a bit withered; a slight pressure may cause them to open; meanwhile, the filaments on which

with the wheat seed. The grape fruit, which interests us most because of the precious juice it contains, is, properly speaking, only a kind of envelope, a succulent capsule designed to retain the seed, and comparable to the glumes of wheat themselves.

they are borne elongate; the anthers, forced to project beyond the glumes, which immediately envelop them and the nascent kernel, come together and unite, because the kernel above which they extend no longer holds them apart; they extend into the light by way of the upper extremities of the two glumes that cover them: but the glumes, being well joined and closely applied one against the other by a flexible movement, closely enclose the stamens and the berry; in rising, the anthers ultimately succeed in imperceptibly pushing aside the two glumes, slightly exposing their tips to the light; the [anthers] come out, but in doing so, they undergo a slight pressure, which cracks them and causes them to emit their dust [pollen] that falls directly on the pistils of the embryo and on the nap surmounting its head.

Could I observe this admirable mechanism without being convinced that the nascent seed [ovule] depended on a sort of perfection of the stamen dust [pollen]; and that the liquid contained by the [pollen grains] was designed to enter the immature kernel, since, after a few days following the fall of the dust on the pistils and the tuft, it is no longer visible there and must have been dissolved?

Could I observe that the bunted heads became noticeable and abundant at flowering time, up to this time appearing healthy and whole; that the behavior of the stamens is interrupted; that the anthers are open, withered, and empty; that the kernels by degrees soon blast—could I consider these different effects in their ensemble without believing that there is a marked relation between the alteration of these nascent kernels and the moment when they should be fecundated? Was it possible, in short, to observe a prodigious number of insects on the wheat; find there their excreta and slough; see there their progeny, which they never leave except in places where the young insect, unable to move away, may find subtenance; was it possible, I say, to consider these little animals, making their common sojourn in the heads of wheat, without suspecting them of being the principal cause of some depredation and of destroying, in order to obtain food, the very place that had served them as a retreat.

I noted particularly the interrupted rôle of the stamens and the failure of fecundation consequent upon the damage they [the insects] caused in the bunted kernels, although

this defect was the only untoward condition I observed there. But I was strongly led to believe that the insects, living in great numbers in the wheat heads, and abundantly multiplying there, must cause a derangement in some part of the plant, injure it there, regardless of its nature, and pass with some probability as the remote cause of three major diseases that we shall consider.

I have already stated that the first thing by which I was impressed, in observing the shooting wheat plants, was the inverse rolling of the leaves and the malformation of the stems. It did not seem astonishing to me that the black insects, not yet finding the tender food that the immature heads must furnish them, attach themselves, while waiting, to the then very delicate culms and leaves; that they deranged their texture, and that the stems they would have attacked became misshapen from the fact that that side of the stem damaged by the insects makes less growth than the side they had not touched. This circumstance appealed to me: (1) As the consequence of a derangement within the imperceptible canals that conduct the sap to the head. (2) This point alone gave me occasion to consider the theory advanced by Mr. Wolf, to the effect that these false grains become blighted and die because their malformed vessels stop within themselves, as Mr. Wolf says, the flow of the nutrient juice and become a cause of disease.

I went farther, in suspecting that the aborted grains were the result of the more or less evident derangement that the black insects had been able to produce in the young stems. Because of these same animals [insects] I suspected also some evidence of damage in the smutted wheat plants. It seemed to me that they had been able to alter the tender and delicate epidermis of the nascent grain, of which, indeed, nothing remained in the smutted plants: I surmised from that that the farinaceous juice, being neither retained any longer by its enveloping membrane nor augmented through the base of the grain, finally dried, blackened, and became an impalpable dust.

The black dust of the smutted heads, viewed through a microscope, appears to be of the same nature as that of the bunt balls; the particles are simply smaller because the grain whence they come has not taken on so much growth as has the bunted kernel. The particles of the dust from the

Microsc. observ. p. 103.

smutted grains as well as those of the bunt balls are of equal size and perfectly round. I have not at all been able to agree here with Mr. Needham, either as to the primary cause of the dust of bunted kernels, as well as that of the loose-smutted grains, or as to the characteristic shape of the dust particles. Here is what he says: "Smut is a disease of wheat the farinaceous internal matter of which it destroys, and introduces in its stead a foreign material that blemishes and blackens the grain, at least externally. This material is a black and very fine dust but its particles, when seen under a microscope, are not of uniform shape." The English naturalist's next statement relates to wheat affected by ergot.

I shall not pause to show how Mr. Needham appeared to depart from the natural order in contending that in the disease called smut the farinaceous substance is replaced by a foreign material; I shall assert only that, by means of an excellent microscope, the dust particles of both bunted and loose-smutted grains appeared to me to be of uniform type. I shall state, moreover, that Mr. Needham's observations inspired me to examine them even more minutely than I otherwise, perhaps, would have done; and that I have always found them invariably of the same shape.

But how did this uniformity of particles in kernels reduced to a black dust come about whilst the flour produced by sound grain consists of quite unequal particles?

Perhaps one might reply that the farinaceous juice is composed of a multitude of small globular particles, whether they subdivide and become rounded just as one finds them or whether they are formed thus and become rounded in passing through the canals the structure of which is cellular and where, so to speak, they take shape: perhaps one might dare to venture the further opinion that this sap, composed of globules, once it reaches the interior of the grain, retains the form of its particles until the liquid from the staminate dust [pollen] impregnates the grain, makes contact with the particles of the farinaceous juice, and causes them to grow unequally; while the particles of the farinaceous juice of the bunted kernels or of the loose-smutted grains do not change in form because they do not share in the fecundating principle and contain nothing of the liquid contents of the stamen dust.

I fully admit that I am quite far from adopting unquali-

fiedly my own surmises and from going to the bottom of a theory followed in detail in which I disclose the manner in which insects may damage the wheat plant and cause the diseases to which it is subject; but I have not been able to abstain from entertaining a new idea, intimately related to my subject and derived, so to speak, from my observations. I have long adhered to it, especially in taking note of the fact that every day, under our eyes, plants damaged by insects grow only indifferently, or show some singular abnormality, or perish outright. I here present a fact that, in itself, at first contributed but little toward leading me to accept the insect hypothesis, because of its analogy with those insects presented on the diseased wheat plants: it is excerpted from the Natural History of the Barbados Islands.

Mr. Griffith Hughes, Member of the Royal Society of London, and author of that Natural History, says that sugar cane is subject to a kind of smut. He asserts that this disease is caused by swarms of little insects, which, when young, are scarcely perceptible; that these little animals, whose food is the juice of the canes, wound the still tender shoots, occasion there some little protuberances, derange the movement of the sap, and sometimes kill the plants. He says that the beginnings of this disease are difficult to recognize. He adds that smut is a periodic malady and that a canton, in which it has been observed one year, is affected by it the next year. This led him to presume that the insects deposited their eggs in the places frequented by them and made provision there for a numerous progeny.

Mr. Griffith [Hughes] believed that in attributing smut to insects one might explain very easily the peculiarities observable in this disease. I thought as he did and, to return to our subject, I did not sense the difficulty of reconciling the insect hypothesis as a cause of disease in wheat with the different observations I had given as principles and that have served me as an established basis for the discussion of all the opinions I have reported.

Before proposing with confidence any remedies against the major diseases of wheat, it would be necessary to understand thoroughly their fundamental cause, to observe it accurately; moreover, it would be necessary that this cause be of a nature to be foreseen by men and prevented by every

precaution that prudence and genius, aided by observation, may suggest to them.

I have already said that I am far from believing that I have touched the decisive point in attributing to insects the blast of the seed. The explanations I have given are not without obscurity, even though taken in their natural sequence; and I feel that it would be just to require of me more detailed, more positive proofs. Moreover, my hypothesis, which contains nothing contrary to the facts I have recorded, might not be in accord with other facts that I have recorded, might not agree with certain other facts that have escaped me. Possibly we shall conclude by considering the black dust of the bunted grains as the end product of some hidden internal contamination, differing little from that that one observes in Lycoperdon or the puffball, the intact envelope of which encloses a black powder, which, viewed under the microscope, presents an infinitude of round and coequal particles. One may, perhaps, be able to confirm this idea in recalling what travellers have said about the famous Apples of Sodom,[2] which, with a fresh, beautifully red skin, were inwardly like Lycoperdon.

Whatever it may be, I shall terminate here my first observations on the diseases of wheat. I shall pass without mention everything that I prescribed for the husbandmen, on account of the insect hypothesis, in order to prevent the diseases of wheat, or at least to lessen their effect. The precautions that I indicated had this advantage that they could be observed without their entailing more care or putting the farmer to greater expense. I even insisted on certain precautions that careful farmers never fail to observe, and I furnished, so to speak, only an hypothesis into which fitted conveniently methods employed to prevent the diseases of wheat.

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[2] One may see near the Dead Sea, a day's journey from the mouth of the Jordan' and on the west side, many of these trees of Sodom of which the writers of antiquity make mention. They are the height of fig trees and their wood resembles that of the fig tree. Their leaves approach the verdure and form of those of the walnut tree and their fruit is similar to that of big lemons, of which they have the shape and color but possess neither the firmness nor the quality: its beauty tempts and attracts the hand, but on seizing and pressing it, it yields and appears to be empty, like a sponge full of air. Foulcher de Chartres, who in his day also visited this country, speaks of these trees in this manner: "I saw there," he said, "something like apples in trees, which, on breaking the skin, I found only black and powdery within." *Voyage nouveau de la Terre Sainte, livre 4e. page 380.*

Flavius Joseph tells also of these same fruits of Sodom: He says that they appear good to eat, but that they crumble to a powder on being touched. *Guerre des Juifs contre les Rom., livre 4e. Chap. 27.*

Vertitur in cariem viridi sub cortice succus

It is well known that, being little satisfied with my first disclosures on the cause of the diseases of wheat, I felt more than ever compelled to investigate this interesting subject and neglect nothing that experience might teach me.

Although there was every appearance that the fundamental primary cause of the disease of wheat that constitutes the principal thesis of this memoir depends neither on the nature of the soil in which the seed is sown nor on the kind of manure with which the soil is enriched, nor on the time of seeding, nor on the particular kind of seed sown; although I am much inclined to believe that the most common preparation of the seed, though helpful, does not go straight to the root of the trouble; I desired to place myself beyond reproach; I sought only to put myself in a position of speaking solely from the facts, should I yet have occasion to explain myself on this subject; and I have resolved to employ experiments as much as possible in making deductions.

Consequently, last October, I chose an undisturbed plot of ground, suitable for wheat, an area 540 feet long by 24 feet wide. I divided my plot crosswise and made 5 equal parts, each of which was 108 feet long. The first of these parts had been manured with pigeon droppings; the second with the freshest sheep manure I could find; the third with night soil; the fourth with the manure of horses and mules; the fifth was not manured; after some tillage, the seed was sown, just as it was, after a year's storage. I gave the entire tract 4 ordinary cultivations: it received a fifth, of a kind, at the moment of planting, which was done by means of the hoe.

As my plan was to employ seed from different cantons, to select the best and the poorest, vary the preparations, and make my sowings at different times, I subdivided each fifth part of my entire ground into 6 equal parts each consisting of 4 beds, each bed being 18 ft. x 5 ft.; the remaining 4 feet of the total width of the tract was distributed among the beds and served as alley ways. This plan of subdivision gave me 24 beds in each fifth part of my ground, a total of 120.

I did my seeding on 6 different days: October 16, 22, and 27 and November 3, 10, and 22, 1751.

I stated that my planting was done with a hoe. This

easily managed implement made it possible for me to sow in each bed as many rows of wheat as I wished; I sowed 6 in each, on the first, the third, and the fifth days; I sowed only 5 on the remaining 3 days: thus the first rows were 10 inches apart and the others, 12. The furrows made by the hoe were hardly 3 inches deep.

Each seeding day, I endeavored to sow 4 beds of each fifth part of my ground. Thus, every day, where I sowed, I worked over each kind of manure, and the day's end found 20 beds done.

Each time that I went out to sow, 4 little ticketed bags were carried out, the first of which contained wheat treated with simple lime or lime mixed with saltpeter, according to the day of seeding; the second bag contained nontreated wheat; the third contained wheat treated with lime and sea salt; and the fourth, wheat blackened with the dust of bunted kernels.

I have always taken care that the treated grain, as well as the nontreated, came from hand-picked bundles from which all bunted and loose-smut heads had been removed. The purposely smutted wheat did not require the same precautionary measures. I have frequently collected it on the market from among the poor, shrivelled, or tarnished wheat. The good seed that I used in the 6 sowings came from different cantons and was furnished me by farmers with whom the grain was popular.

In sowing my ground, I used only 12 pints of seed, or thereabouts; which amount, after making all deductions, represented about half the quantity of seed used by local farmers in seeding an equally large area of plowed ground.

Since my two neighbors did their seeding at the same time I did mine, I shall have opportunity to examine something that has long puzzled me; that is to say, the evident loss, in the ground, of a part of the seed, which the plow buries too deep; since, at the rate of 4 and sometimes 5 bushels of wheat per arpent, broadcast, one commonly gets a return of not more than 20 to 25, and, in good years, 30 bushels per arpent.

In addition to the sowings made in the open field of which I have just spoken, I prepared some beds in a garden in which I sowed wheat. Certain of these beds were left in their natural condition. Others were fertilized with poultry

droppings or with hog manure. I even made one bed, 18 ft. x $3\frac{1}{2}$ ft., out of woodland moss and without communication with the soil. I sowed some wheat in this moss and it is now (February 15) almost as advanced as my other sowings, although it was not sown until December 9.

One may see from all this detail that I shall be able to depend a little on the observations that the wheat of my little tract and that of my different beds will enable me to make; and that from the large number of combinations resulting from my sowings, some light may be shed and will find place in this memoir should I be so fortunate as to have some of it impinge upon me.

SUPPLEMENT

THE experiments that a man, himself, conceives and performs have something to their advantage. They are valuable to him who performs them, hence he follows them closely. These experiments, with which he becomes very familiar, strike him from the different aspects in which he may view them and afford him opportunity to multiply his observations by increasingly stimulating his curiosity. The wheat plants produced by the plot, the distribution of which we have noted, having not yet acquired their full growth, I am unable now to give the result of my sowings. Already, I see several facts to put down concerning the major diseases of wheat, others to develop, some reflections to be added.

I am pleased that the Academy will be agreeable to accept a supplement in which these facts will be presented in greater number and in a manner more detailed than in my memoir. They will enable me to rectify my ideas on an important point and will make more evident than ever the inadequacy of all that has hitherto been said in an attempt to establish the true cause of the diseases of wheat.

I have observed these diseases in their initial development in so far as it has been possible. I have caught the rachitic condition in its incipiency, so to speak, at a time when the wheat stem begins to form; I have followed the progress of this disease throughout its entire development; I have recognized bunted wheat plants several days in advance of their flowering; I have been fortunate enough to find some heads destined to become loose smut, although

tightly enclosed within their sheaths; I have noticed that such heads, after having begun to show a kind of moldiness, rot insensibly and finish by changing into dust. But, let us present the facts; they will constitute in themselves the series of my observations and will serve me in giving an historical account of them.

Facts concerning blasted wheat plants

I. Wheat heads in blasted condition ordinarily occur on stunted culms of a bluish hue, the leaves of which are curled.

II. The stunted condition of the culms obtains from the time they are only 3 or 4 inches tall.

III. When the stunted culms are short they are flimsy and of a yellowish color; their leaves are of the same color and a bit curled and withered.

IV. As the stunted culms grow, they assume a greenish color and ultimately acquire that characteristic bluish hue peculiar to them: their leaves, always remaining curled, also become bluish and never possess the vigor and strength of healthy leaves.

V. The stunted condition is not one peculiar to poor, impoverished land; I have observed it on good soil in the midst of the finest wheat plants; and sometimes I have vainly sought it where the wheat was very thin and appeared to be in poor condition.

VI. The roots of stunted plants have seemed to me to be a bit altered: Many of them were woody; such roots were not so completely and generally covered, as are those of healthy plants, with that spongy, velvety cork the function of which is without doubt the moistening of the ligneous part.

VII. Rarely does the same stunted wheat plant bear both completely sound and wholly blasted heads; when that happens, the good heads are borne by erect culms whose leaves are but little curled.

VIII. The kernels from stunted plants are like young peas and show one, two, or three very distinct apices.

IX. The pea-like or blasted kernels that ordinarily occur on stunted and curly-leaf stems, are sometimes found along with bunted kernels in the same head, borne on erect culms whose leaves show no curling.

X. A stunted culm sometimes bears sound kernels and pea-like kernels in the same head.

XI. The blasted kernels are very weakly attached at the base of the glumes; they become easily detached when the glumes are pushed aside for their observation.

XII. One very occasionally finds a blasted kernel retaining one or two of its stamens; I have never seen any that bore all three.

XIII. The blight of the kernels is sometimes entirely consummated before the head is out of the boot.

XIV. Occasionally, when the heads of stunted plants have been exposed to the air, the glumes begin to fade and the blasted kernels blacken and become dry.

XV. One sometimes finds certain stems of wheat that, although stunted, are quite erect, very tall, and bear curled leaves only from the 3rd and 4th nodes. The heads contain blasted kernels in one, two, or three spikelets; the blasted kernels being enclosed by glumes containing a white grain accompanied by one or two yellow stamens; the white grains bearing three very green stamens and apparently destined to function normally.

Facts concerning wheat affected with smut [*loose smut*]

I. Smut [loose smut] is a disease that commonly extends to all of the heads of a given plant.

II. It sometimes happens that a smutted head is produced by a plant that puts out several healthy heads; or that a given plant produces several smutted heads and a single healthy one; or that one and the same plant may produce several smutted heads and one bunted head. A smutted head may sometimes be only partly smutted, the remaining portion of the head bearing either bunt balls or sound kernels. In a single head there are sometimes only one or two smutted kernels.

III. It is easy to detect a smutted head, even though it be tightly enclosed by the boot and exhibit no outward signs of its presence. Smut begins with a sort of mouldiness observable on the still green head. The head gradually blanches; some black spots form on the glumes; little by little they break down, retaining always a certain degree of humidity: the entire head finally breaks down, dries out, and becomes a black dust, with the exception of the rachis, the awns, and some remnants of the glumes, which dry up without being affected.

IV. When the head begins to show signs of smut it may

be two inches long or thereabouts; the nascent kernel is then hardly noticeable.

V. It is seldom that a smutted head contains either sound or bunted kernels; I have yet found no smutted head that bore aborted [blighted] kernels.

VI. The roots of wheat plants affected by smut seem to have been altered, the ligneous portion being deprived of its cortex in some places. These roots bear few root hairs: otherwise, they have the same consistency and the same elasticity as those of healthy plants.

VII. The upper part of the stem of smutted plants, from half to three-fourths of an inch beneath the head, is commonly not very straight and presents a slight, more or less evident sinuosity; if one squeezes the stem at this point, it resists and scarcely yields, at all; if, at two or three lines beneath the head, one cuts this same stem horizontally it will be found almost entirely filled with a medullary substance, one will see in the middle a tiny cavity: if, instead, one presses with the nail this same part of healthy stems one will feel only a little resistance. The parts come together easily; also, if this part of the stem is cut horizontally, it presents a lumen proportionate to its size which joins up with that obtaining throughout the culm: it appears that at the top of the stem of the smutted plant there is a sort of obstruction.

VIII. When from a wheat plant there arises a smutted culm and when from this same stem there arises another that is totally dependent on it, this secondary stem always is affected with smut. This observation has been true also of both blighted and bunted plants.

Facts concerning bunted wheat plants

I. Ordinarily, all the heads of a bunted plant are entirely bunted; in the same plant, as many as eight bunted heads have been found.

II. Some bunted heads may appear among healthy ones borne by the same plant.

III. One sometimes sees heads that are partly bunted and partly sound.

IV. A head attacked by bunt sometimes presents several gradations of this disease when caught in its incipiency. The head contains some apparently sound, whitish kernels

each accompanied by its three green stamens; some kernels that are slightly blotched with green of which one or two of the three stamens are yellow-tinted; other kernels that are almost wholly green, the stamens of which are nearly all yellow; and finally some kernels that are entirely green and in their last stages of disease, the stamens of which are quite yellow. It sometimes happens that in this sort of head, where bunt has produced a more or less marked effect one finds a kernel without any defect and accompanied by three green stamens, although this sound kernel may be in a spikelet along with an entirely bunted kernel, the stamens of which are yellow.

V. Bunted heads never blossom, or, if some of the kernels of such a head bears stamens projecting above them, they blight and are of none effect.

VI. When from the same plant there grow several bunted heads, the presence of bunt is noticeable in the youngest heads, even though still in the boot; the stamens are yellow and drooping; the embryo assumes here and there a greenish tint.

VII. The heads attacked by bunt, even by the time the stem is full-grown, are soft and never possess the consistency of sound heads. The leaves of the stem bearing such heads are dark green and spotted with yellow.

VIII. In general, in all the bunted heads the three anthers, although collapsed and supported by filaments that, themselves, are very slender and withered, are disposed about the kernel precisely as they should be; their triangular position is accurately observed.

IX. Bunt balls are sometimes found in otherwise smutted heads; or are mixed in the same head with blighted grains.

X. The roots of bunted wheat plants, when one pulls up a stem, do not seem to have the same firmness, the same strength, the same hairiness, and so many minute branches as do those of healthy plants; it seems that the roots of bunted plants have undergone some change.

XI. The glumes of bunted heads bleach and become dry when exposed to the air for some time. The glumes of healthy plants of the same age do not fade until they begin to mature.

M. Duhamel seems persuaded that smut and bunt are

virtually one and the same disease that finally comes to fruition as so many smutted heads. To the reasons he advances in support of his opinion we might add one that has escaped him, one that is, nevertheless, striking enough to give some likelihood to his concept; that is to say, *it sometimes happens that one and the same plant is affected with bunt and with smut; that one and the same head may be stricken by both diseases together.*

M. Duhamel has not referred to the disease described in the foregoing under the name of blighted wheat: thus he has not been able to explain the connection and the relation one would be tempted to picture between this disease and the two others he observed. If the statement I have added to that of M. Duhamel, in order to establish a sort of analogy between the occurence of smut and that of bunt, were plausible, one might use it with equal justification in maintaining that the blight of wheat may be obscurely related to smut and bunt, since the bunted heads sometimes contain blighted grains and the leaves of a plant attacked by bunt sometimes are curled, a frequently observed and common indication of blight. Bunt balls are sometimes found in an otherwise smutted head of wheat; blighted kernels are sometimes found among bunted kernels in the same head; the connection is obvious; blight depends on bunt; bunt depends on smut; I omit here several particulars common to the three diseases of wheat; they appear in the perusal of facts that I have assembled. However, I would not dare to consider the connection, the obscure identity of these three diseases, as being very constant. There are some varieties that seem assignable to wholly distinct classes; and I am today very sure, through repeated experiments, that whatever factor may be an abundant source of bunt in wheat is not at all a cause of either blight or smut.

M. Duhamel has clearly shown how vain it is to seek the cause of the major diseases of wheat in the influence of mists, in the failure of fecundation, in the shock of bright sunlight impinging on the heads while they are imbibing water, and in the excessive wetness of the seed bed.

I shall not stop to bring together more detailed facts than those I now give to disprove the theories that several naturalists have advanced. It is plain that they are not supported by repeated observations and that they have by

no means brought together the extremely numerous and varied effects that an experimental consideration of the subject discloses. It was, however, an excellent route by which to arrive at the determination of a cause that satisfied all or that at least left but few obscurities. I believe I should only note the fact that there is no likelihood whatever that smut and bunt are caused, as Mr. Wolf asserts, by the anomaly of kernels in which the sap has been stopped and has stagnated. From several of my observations it has been decided that the bunted kernel possesses its natural shape before it becomes diseased, that it retains its shape quite well after completing its development, and that the smutted head, when it begins to break down, contains scarcely perceptible kernels and shows nothing by way of a malformation. Mr. Wolf has considered barley only, and upon this plant has based his conclusions for all those plants that are subject to bunt and smut.

It will, moreover, be worth while to apprize you of the fact that I have to-day some new evidence to show that the great abundance of water with which the culms of wheat are filled is the cause of neither smut nor bunt. In order to be more certain, Mr. Tull's experiment has this year been repeated under my observation by an attentive and exact person. One wheat plant produced by a single kernel was watered three times per day and was nearly flooded with water. I had transplanted this plant in the same earthen pot as that whose dimensions have been mentioned elsewhere in my memoir. This plant, which grew prodigiously, is already bearing several fine heads and I anticipate seeing very few if any bunted heads among them.[3] I shall add here that the wheat grown in that bed of pure moss referred to towards the close of my memoir is now a thing of rare beauty. It has produced a large number of very healthy heads some of which are as much as six inches in length. Throughout the whole extent of this bed I have thus far observed only two bunted heads.[4]

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[3] This plant has produced 89 culms, the majority of which are three to four feet high. Although among a large number of very beautiful heads there may be several poor ones, others that may be slender and shrunken, none, however, is affected with bunt and none manifests any blight or loose smut.

[4] I made this observation on the 18th of June, 1752. It will readily be seen that if I noticed then only very few bunted heads in this moss bed it was because the disease was slow in manifesting itself in the part of this bed where it should pre-

Even though one remain persuaded that certain fertilizers, certain soils, in themselves, contribute to the disease of wheat and render disease more general, one will at least admit that they are not the primary cause, since wheat plants come up from the pure, washed moss and, with every possible attention, only to be found affected by the most important of these diseases. On the other hand, if excessive moisture is the source of the diseases of wheat one should frequently notice them in that moisture that has produced the moss, since it is nourished only by water and languishes when watering is neglected.

I would hardly be able to state with certainty at this time that the nature of fertilizers, considered solely from the standpoint of animal excreta, lack of treatment of otherwise well-selected seed, the nature of the soil, earliness or lateness of sowing, condition of the weather during seeding; I would, I say, hardly be able to assert with certainty that all these exert no influence on the diseases of wheat and disclose facts that always leave an observer in doubt. After harvest, I shall have occasion to return to this subject and enter into such detail as it is now impossible to give with certainty.

I would not, however, know how to pass in silence a matter that will be a part of this account; i.e., that in examining the different beds, the plan of which has been laid before the Academy for examination, I was surprised to note how all those in which I sowed defective seed or seed purposely blackened with the dust from bunted kernels, contained a great quantity of bunted heads, notwithstanding the facts that these beds were fertilized with different manures, that among them there were six that had received but one kind, and that the seed sown there varied according to the canton whence it had been obtained and the time it was put into the soil.

Last year I did not know exactly where the black insects, considered in my memoir, laid their eggs. I have but recently discovered that these almost imperceptible eggs are deposited along the rachis of the head, at the base of the pedicels supporting the spikelets: they are easily made

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dominate, and it attacked there in the course of time a large number of heads that subsequently showed the same symptoms and precisely the same developments as did the wheat plants that drew their moisture from the soil.

out by means of a magnifying glass: ordinarily, they number two or three. I have seen as many as eight at the base of one pedicel; they are much more commonly found on the good than on the bad heads.

I have stated that the incidence of bunt might be occasioned by these same black insects and that I suspected them of damaging the stamens, thereby causing a failure of fecundation and a consequent deterioration of the ovule. Following some exact observations, I have retraced my steps; I have reflected on how several bunted heads were in that condition, although snugly enclosed within the boot and thereby inaccessible to insects. I have remarked that since the disposition of the stamens around the bunted kernels, sometimes collateral, sometimes intermediate, is precisely that fixed by nature, it necessarily must follow that the disease did not result from any external derangement. I felt that insects, however minute they might be, would, of necessity, leave more or less noticeable indications of their ravages around a very delicate kernel if they found there any nourishment, and that at least the stamens would not always be in their natural position. These reflections, in addition to several observations, which it is useless to recall, have caused me to conclude that both smut and bunt are wholly internal diseases; that when the head emerges from the boot, either the disease has attained already its full development or it will come to maturity little by little in the head exposed to the open air without any external contributing cause.

Sixth observation made on blighted wheat plants. Sixth made on smutted wheat plants. Tenth made on bunted wheat plants.

It is necessary then to seek a cause the effect of which is made manifest within the very interior of the culm, a cause that leads to a plausible explanation of all the capriciousness one observes in the three diseases in question.

In spite of a very exacting study on my part and an examination of wheat in the different stages of growth in which I have been able to secure it, I noted, in the light of some of my observations, no other cause of its diseases than an alteration within the roots whether through the many-times cited work of black insects or that of other creatures[5] unknown to me.

[5] I have sometimes discovered among the roots of wheat plants some insects that appeared to me to be a sort of plant louse. Once I saw them when they were quite small and in sufficient quantity to induce me to believe they had originated there. On the same roots where I saw them, there was a plant louse much larger than they

The little black insects that one finds associated with wheat as soon as the head is formed, live, according to every indication, and remain hidden, underground during the winter. With the first fine days of spring some of the insects begin to come out and spread to the most vigorous plants: I have seen them in the month of April. I would not depart from the natural order, if, after having noted a slight alteration in the roots of diseased wheat plants, I suspected that these animals obtained nourishment from the sap of the roots and that they, in slightly deranging the tissue, inoculate it with a sort of virus that diffuses throughout the plant and produces its major effect on the head. I find myself drawn to this idea by two apparently correct observations.

1. In examining wheat plants during the winter, I noticed several had died. I did not know why. It was only after pulling up some of them that I noticed the roots were injured and that the bad external condition, by which I was impressed, resulted from the alteration that the roots had undergone.

2. The stunted wheat plants are early recognized; on reaching a height of 3 or 4 inches they are yellow, wilted, and soft; their leaves are curled and withered. These same wheat plants, on resuming growth, become bright green and complete their growth by assuming a dark bluish green. One may assume that the stunted plants have suffered a considerable reaction in their roots during the winter, that they have put out new ones with the beginning of spring, which supplied them with a little vigor; but that in these same sick plants, being yet unable to find in these few new roots the resources of a completed growth, their culms and leaves have languished and their grain become blighted.

Although I have carefully washed a very large number of wheat plants attacked by bunt, or smut, or that were in a blighted condition; although I have examined by means of

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and, which, without doubt, was a female. These insects should be followed up, but that is a task. When one pulls up a wheat plant with all its roots, they are covered with soil and are all mixed up: it is difficult to observe the very small creatures by themselves hidden among the soil-covered roots and always exposed to destruction by the hands of the observer when he washes the roots in order to hunt for them. M. Réaumur, whom nothing escapes, says that there are some kinds of plant lice that attach themselves to the roots of various plants and feed on them. Among other roots he cites those of oats, which, as is known, is one of the plants subject to smut. Mém. sur les Insectes, Tom. 3, pag. 340.

a hand lens or with a microscope the parts of roots that seemed to me to be injured, I have seen nothing that fully satisfied me. Moreover, the damage that may have been done by such small insects as ours to the roots that have grown since the time when it was believed they were attacked, roots covered with a spongy cortex, pitted with an infinite number of pores, and provided with a kind of nap, this damage may well be of such nature as to escape the most painstaking search.

I have noted above that all the beds of my plot in which I sowed bunt-inoculated seed without any preliminary washing; I have, I say, noted that these beds, thirty in number, contain much [smut] although those alongside have very little or none at all. Might not, then, the diseases, smut and bunt, be heritable? The observations of M. Duhamel, the experiments he reports, do not entirely eliminate this concept. Do the bunted kernels contain some virus? Might not their black dust be impregnated therewith to the point of communicating it to the seed? Might not this malign dust produce its effect on the good seed by penetrating it with its virus when the seed begins to swell in the ground and when its germ begins to grow? Are not the wheat plants grown from smutted but subsequently washed seed less likely to engender smut than those grown from seed of the same sort that had not been washed? Might not this be because the washing removes the black dust from the seed and thereby prevents the evil effects that it, the dust, would produce?

Whatever it may be, this considerable quantity of bunted heads that I have noticed only in those beds sown to inoculated seed, deserves special attention. I shall not fail this year to sow again some smutted seed alongside other thoroughly clean seed and select the best heads. It would be helpful if several people would volunteer to carry out the same experiment in several different cantons. Perhaps it would result in our knowing to a certainty whether the dust of the bunt balls is a germ of disease; whether the smut disease is hereditary.

EXPERIMENTS

AND

CONSIDERATIONS

RELATIVE TO THE DISSERTATION

ON THE CAUSE

Of the Blackening and Destruction of Wheat Grains in the Head and on the Methods of Preventing these Unfortunate Circumstances

AFTER the details into which I entered in the Memoir, which I have had the honor of presenting to the Academy, and in the yet more detailed supplement I have added thereto; after these details on the different diseases of wheat, on the special classes to which they should be consigned, and on the specific symptoms that characterize them, I should be permitted to summarize the three principal diseases and the names I have given them.

SECOND PART. 1751, 1752.

I have signified as blighted those wheat plants whose culms are ordinarily a little twisted and more or less stunted, bearing blue-green leaves that are somewhat curled, green kernels terminating in one, two, or three minute bosses and quite similar to a pea in the early period of its development within the pod.

I have given the name *blasted* [smutted] wheat to those plants of which many of the heads appear to have been destroyed by fire; in fact, there appears to be left of such an attacked head only a black powder. Sometimes, little white threads escape from the degenerate mass and always a kind of central column along which the grains were arranged.

I have finally designated under the name *bunted*[1] wheat those plants the disease of which should properly be made the object of our investigation; those plants, the heads of

[1] This term, as well as that of bunt, will so often recur in what follows that I shall sometimes employ in their stead the term smut, already in use, although too general in its application to characterize clearly the disease in question.

which, unfortunately too common, do not blossom, although apparently well-formed and equipped with stamens. Their kernels, deteriorating gradually without losing their natural shape, finally become, internally, a mass of soft, blackish dust of unbearable odor.

Although the experiments I made in 1752-53 throw the most light on the disease of bunted wheat, which will be the principal subject for consideration in this supplement, I have, nevertheless, thought it would be well to review what I did in 1751-52. Aside from special facts brought out in those experiments, of which it is well to take account, it will be perceived that they shed the first light to guide me in subsequent experiments. It will be seen how an effect that I had not anticipated, even though I had worked to bring it to pass, disclosed to me a cause of disease, which, until then, had not been suspected.[2] This cause, obvious as it is, did not appear to be of any consequence to wheat, being, as it seemed, out of all proportion to the marked effects we shall subsequently witness. The reader, in considering the essential objective to which I applied myself, will be the better able to judge of the progress and of the correctness of my observations.

See project I.

At the end of my memoir there was an account of the preparations I made in 1751 incident to the experiments concerning which I shall have something to say the following year. It has been observed that the plot on which I carried on my work had been under cultivation and was one of the best for wheat culture in the canton. It has been noted also that this plot was 540 feet long by 24 feet wide, and that it was divided lengthwise into 5 equal parts; that the first part was fertilized with piegeon droppings, the 2nd with very rich sheep manure, the third with night soil, the fourth with horse and mule manure, and the fifth received no fertilizer of any kind.

Care was taken that each fifth part of the plot, 108 feet

[2] It is true that M. Duhamel has had some slight suspicions about the possibility of bunt being a hereditary disease. Some authors, without reference to their precise experiments, have, in general, counselled against sowing *tipped* wheat, *soiled* wheat. But one should observe here that the really useful advice of these authors and the opinion of M. Duhamel share the same principle, namely, the supposition that the germ of the disease resides in the grain itself; that it is essentially internal. It does not appear that they suspected (which is the essential point he needed to grasp) the cause of the malady to be external to the grain and dependent on some particles of dust distributed over its surface membrane.

long, was divided into six equal parts, each composed of four beds 18 feet long by five wide. The four remaining feet, extending all the way across, were left to serve as alley ways between the beds and as plot borders.

It has been noted that my seeding was done at different times, i.e., on six different days, separated by intervals. These seeding dates were the 16th, 22nd, and 27th of October and the 3rd, 10th, and 22nd of November. It also has been observed that I sowed the seed in rows at a depth of two or three inches, following somewhat the method of M. Duhamel. Care was taken to place the rows just 10 inches apart. It has been seen, too, that in planting 20 beds each day I covered those that had received each kind of fertilizer and also the unfertilized portion; that the first kind of seed I used was either dirty, poor, and shriveled wheat, obtained on the market and in different sacks, or wheat that I had purposely smutted with the dust from bunt balls; the three remaining sorts of wheat employed in my seeding consisted of choice seed obtained from four different cantons and treated with lime and a saturated aqueous solution of common salt or left in its natural state or treated merely with lime or jointly with a saturated solution of nitre.

According to the order observed in sowing, the dusted or purposely smutted wheat was sown always in the first row; the seed treated with lime and salt, in the second; the non-treated seed, in the third; and that treated merely with lime or with lime in combination with nitre, in the fourth row. This precaution was useful to me; each kind of seed thus planted in 30 consecutive beds, presents a singular prospect to those who observe them but have no idea of the cause, and, even to me, who has commenced to have some inkling of it.

I pass without comment the less important observations I had occasion to make on the condition of my beds by the close of 1751, and at the beginning of 1752. It really was not until the early part of April that anything offered worth noting in my journal; even the first observations I shall here narrate will offer nothing of much interest.

On the 9th of April, 1752, I noticed that several wheat plants were dead, while others were very small, spindling, and of a sorry aspect. I pulled up some of them and found them partly rotten. It seemed to me that they had suffered

from having been buried a little too deep, either through faulty seeding or because they had been covered through the removal of ground underneath by mice or moles.

On the 11th and 12th of April I gave my beds a light cultivation and pulled up all the plants that had grown between the rows during the winter. This work gave the wheat plants new vigor.

The 22nd, everything was going well. The first beds of the portion of ground fertilized with night soil were in a very flourishing condition and resembled most excellent patches of rye. I gave particular attention to a large number of culms the leaves of which were frayed; I pulled up five or six of them, but saw nothing unusual about them beyond this slight derangement in leaf tissue; I, however, selected one of these culms with torn leaves and noted some yellow spots. I opened the sheath of a leaf and found there a greenish and minced material that looked as though it might have been intended for some kind of sauce. I examined this stem with a magnifier and this green material appeared to me to be a yet enclosed leaf that some insect had penetrated and had partly broken and eaten: the base of the still very delicate stem was not at all damaged. The little nodes, destined to be lifted one above the other, could be distinguished and at the extremity of the last node could be seen the well-formed head as shapely as the point of a needle and perfectly sound.

One of the first things to attract my attention when the wheat began to shoot was a sort of fluting, very noticeable on a large number of leaves. I was, at the time, much occupied with the insect hypothesis, believing it adequate to account for the grain diseases. At first, I believed that this leaf alteration could be attributed to insects; but, on a more searching examination, I found that this fluting was due to a pronounced constriction of the leaves while they were yet rolled up in the sheath. This sheath, without doubt, not opening so quickly as the growth of its enclosed leaf demanded, caused the leaf to fold back on itself and to assume somewhat the shape of a screw; and, with the development of this leaf, a part of it became lifted in a very regular manner, while the other part, just as regularly, formed a little hollow. I sometimes found leaves still rolled up and thus crowded in the sheath, which showed this sort of fluting

on being released. This derangement of the leaves, non-essential in itself, might be attributed to the force exerted by the plant's growth; in fact, I have noticed that it was common in vigorous wheat plants and quite rare in poor and languishing ones.

Here was the occasion, in order to follow the order of my observations, for me to note down some of the details relative to the three diseases important to wheat; to note their initial, though scarcely perceptible, beginnings; to follow their daily progress; and to characterize them by everything that individualized them, and to assign them to separate classes. This was the occasion to speak more positively about the insects mentioned in my memoir, those animals belonging to the order *Staphylinidae,* which I followed so unremittingly and so uselessly, at least from the point of view from which I considered them: this was the time to note the error into which I had fallen in suspecting them of damaging the wheat grains and of being indirectly a cause of bunt. It was, in short, the moment to recall the observations I had made on the roots of sick wheat plants, being attracted to them by the change that I thought I observed in several among them and being no longer able to find the origin of the disease in the changed condition of the head. But these matters have been presented in my first supplement, and I now shall pass directly to a matter of greater interest in my experiments and that subsequently occupied my entire attention.

It has been noted in the foregoing that in the first row of my plot comprising 30 beds I had sown only bunt-inoculated wheat. I noticed, toward the middle of June, 1752, that there was a considerable number of bunted heads among those 30 beds, while I observed almost none among those of the three other rows. Shortly after, the disease became so manifest in these 30 beds that I finally perceived what I suspected might be the cause of an injury confined to a single row and concerning which I sought to make myself more certain by scrupulous attention to the least thing that might tend to confirm my suspicion.

In carefully examining each of the 30 beds attacked by the disease, I observed that there was more of it where the injury was more generally present than in other beds. I fixed my attention at first on the six beds fertilized with

pigeon droppings (it is always a matter of the first row attacked by smut).

In the first of these beds there was nothing but bearded wheat. Half of the heads were found bunted. In the second, also occupied by bearded wheat, ⅗ of the heads were bunted. In the third, composed partly of bearded and partly of beardless wheat, ¾ of the heads were affected by the disease. In the fourth, devoted to a variety of wheat similar to that in the third bed, more than half of the heads were blasted. The fifth, containing only beardless wheat, was so infected that of 8 heads scarcely one could be found that was not bunted. The sixth bed, devoted solely to beardless wheat, was in as deplorable condition as was the fifth.

I examined just as carefully the beds of the first series in the other 4 divisions. The number of bunted heads was the same as in the first, in spite of the different fertilizers used in the second, third, and fourth divisions, and, although there was none used in the fifth division, this quantity of bunted heads, the same in each division, occurred exactly and for each individual bed, in the same proportion noted for those of the 1st division; that is to say, that in the first bed of the second, third, fourth, and fifth divisions half of the heads were bunted, as were those in the first bed of the first division; that in the second bed of the 4 last divisions, ⅗ of the heads were attacked by bunt, as was the case in the second bed of the first division; and so on for the rest. And this proportion was such that one could judge precisely of the condition of this or that bed of the first series in each division by the condition of the corresponding bed of the first division.

The beds of the second series, for which the seed had been selected and had been treated with lime and sea salt, showed but few bunted heads in the five divisions. It was only by hunting carefully that one could find them. I observed them without difficulty, although few, in the beds of the third series the seed of which had received no treatment. The fourth series was the one in which the disease was least apparent. The beds comprising it had been sown, as noted in the foregoing, to selected and treated seed, treated either with lime alone or with nitre and lime.

This 4th series, where the heads were sound, gave me opportunity to make an observation that is worth reporting.

The ground where I conducted the experiments the details of which I have given, is situated in the midst of a vast field that belongs to different individuals. The nature of the soil requires that it be carefully cultivated, and the same year of my experiments two of my neighbors sowed wheat there. The small portion of ground I occupied was separated from their fields by a mere furrow and I had every opportunity to make a comparison between the condition of their wheat and my own. My neighbor's wheat field, which bordered my first series of beds, was healthy, though the soil was poor; it is true, however, that this series contained almost no smutted heads. The wheat plants of the portion of the field bordering on my fourth series were in a deplorable condition and in great part smutted, whilst those in my fourth series presented a delightful aspect, as much because of the beauty of the heads as because of their healthy and vigorous condition.

I now come to the observation I had occasion to make in examining the wheat plants of this fourth series and those of the neighboring field. This field was sown only after my beds, themselves, were sown. The farmer threw some grains of wheat onto the borders of my plot. He kept close to one side in guiding the plow and covered at the margin of my plots a few of the seeds he had thrown there. The seed germinated there perfectly but carried the defect of the seed of which it was a part. Half of the heads coming from it were found to be bunted. They constituted a hit-and-miss lot of scattered plants alongside of my last rows of wheat; hence it was easy to recognize them as extraneous to the beds in which all was order and where the wheat was in rows, only.

In none of my beds did I notice any blighted heads. I found in them some loose smut, sometimes in one bed, sometimes in another. Those showing the most bunt contained no more heads attacked by loose smut than did those in which there was almost none.

The experiments, the results of which I give here, were made in an open field. It was at first necessary not to depart from the usual order: I did not, however, neglect to try some experiments in the city. In a little garden surrounded by buildings, I put in 4 plots 2 of which were fertilized with hen manure, another with refuse from hogs,

and the 4th was left in a normal garden-soil condition. My object was to determine whether, as some writers assert, these fertilizing agents, reputed to be very ardent, contribute to the production of bunt. In one or two plots where hen manure had been applied, I sowed some selected but non-treated seed; and in the three remaining plots I sowed some of the same sort of seed but treated with lime and sea salt.

With the exception of 5 or 6 heads of loose smut, as many in the beds fertilized with hen manure as in that to which hog refuse had been applied, I observed nothing abnormal. I sought everywhere in vain for some bunted heads: the fertilized plots were as free from the disease as were those where no manure had been applied.

It was otherwise with the plot of moss, mentioned in my Memoir in the course of the experiment I am here citing.

It may be recalled that the moss of which I made use had been got in the woods; that it was carefully sifted before receiving the grain; and had afterwards no contact with the ground, although placed in the earth. Following is the simple method I followed:

On the 9th of December, 1751, I had made a trench in a large well-aerated garden. This trench was 18 feet long, 3½ inches wide, and 9 inches deep. At the bottom of the trench, as well as along the sides, I took care to provide some little outlets to give egress to the surplus water.

I put the moss in this trench and pressed it down reasonably firm. When this bed of moss was finished, I divided it lengthwise into 4 equal parts, taking special care to mark the separations with some little sticks placed on the surface of the moss and attached by their ends to the borders of the plot. I sowed in the first part some seed blackened with bunt. In the second, some selected seed treated with lime and sea salt, in the third, some clean, nontreated seed, and in the fourth, some selected seed treated with nitre and lime. This sowing plan was the same as that I had followed in preparing the plots concerning which I first spoke, and I used for these moss plots a little of the same lot of seed I had left from my sixth and last sowing.

My seed, thus sown, was covered with a layer of moss 2 to 3 inches thick. At first, I sprinkled this plot with a quite considerable quantity of water in order to wet it down well. Thereafter, it was maintained exactly at a cer-

tain degree of moisture by watering once or twice a day, according to need, the watering being discontinued only at times when the plot was covered with snow or during hard freezes: In the latter case, I took care to cover the bed with a layer of straw. The seed, although sown late, germinated quite promptly. The young seedlings became strong in a short time and withstood the rigors of winter, although the surface layers of the moss were frozen at times. These plants shot up vigorously in the spring and followed in their development the progress of ordinary wheat. The heads, however, came to maturity only toward the end of August; some, even, produced on late culms, were gathered without being fully ripe. Aside from this slight imperfection there was nothing to concern me. I was interested only in whether the grain was sound or diseased.

I had the satisfaction of seeing this last experiment tally with those I have already reported. In the first division, where the seed had been blackened with the dust of bunt balls the disease was manifest in nearly all of the plants. Among others, one was found with eleven culms the heads of which were entirely bunted. The beginning of the second division contained a few bunt heads, either because some seeds had come up beyond the little stick that separated it from the second or because the disease had found it easier to progress in the moss and become contagious than had been commonly the case in ordinary soil. Whatever may have been the cause of this slight accident, at least it is certain that there were very few bunted heads in the second division; that they all were neighbors to the first division and seemed to belong to it; that the third and fourth divisions contained no bunt; that this part of the plot was excellent; that there could be noted there some plants bearing a large number of culms some of which bore heads measuring as much as 6 inches in length.

To the simple and purely historical account I here offer concerning my experiments, I shall add only a few comments. They are, themselves, presented in the results I have faithfully narrated at the conclusion of my different operations. The following observations will, therefore, be only a summary of those that contain certain important points and on which I already was informed.

In the large-scale experiments concerning which there

at first had been some discussion, it was observed that the wheat of the field adjoining my first series of plots was healthy, while that of this same series was bunted; and that the wheat of my fourth series was very healthy, while that of the neighboring field was damaged.

Let us not hesitate then to exclude mists as a cause of bunted wheat. They would produce their effect more universally if they really occasioned any such malady; and one would not see it exactly limited to this or that restricted area, since it would depend on a weather condition that ordinarily occupies a quite considerable area and is nearly always unequally distributed.

Let us not hesitate to regard the manures, the excreta of animals, as being incapable of causing bunt in the field where they are employed.

In the first experiments, the pigeon droppings, the sheep manure, the night soil, the horse and mule manure; in the second, the hen manure, the hog manure,—none of these fertilizers, as such, produced any evil effect; at least, none was evident. If, in certain parts of the nursery where these manures were applied, some smut did appear it is necessary to find some other cause if we are to discover the reason for it. They [the manures] would, in fact, have been equally provocative of the disease in the four series of plots if there had been in them anything inherently harmful, and the first row would not have been the only one to show the effect.

But, even though they do not contribute to the bunt, considered, as I have said, solely as the excreta of animals, neither do they possess any virtue as preventives of the disease. There was as much smut in the non-manured as there was in the manured plots of my experimental tract. The diseased plants, it is true, drew much vigor from the manured portions of the plots where they occurred; but there was always present in them the destructive principle—present from their very beginnings and transmitting the disease into the numerous culms produced by these plants. The first series of beds of the fifth division received no manure. If it, on the whole, produced fewer bunted heads than the first series of the other divisions it was because the growth there was less vigorous; but the number of diseased plants was the same in the contaminated portion of each division. I have noted no characteristic difference in the

disease that one could attribute to the fertilizers, regardless of their nature. In one fertilized plot the disease affected a large number of culms belonging to the same plant. In poorer soil there were only one or two, an individual plant commonly producing no more than that many heads: That was the only difference.

That continuity of plots more or less affected by smut, as recorded in the account of my experiments, has, no doubt, been noticed. It has been observed that the seed sown in them was either blotched or blackened with bunt. It was thenceforth suspected that this dust contained a virus with which sound seed could be infected, and attention was then directed to the seed as the starting point of the disease.

This conception of the matter will be all the more quickly accepted after observing another long series of plots bearing almost no indication of the trouble present in neighboring plots, and that series in which the plots varied one from the other, according to the treatment given the seed.

Would not one's attention have been arrested by the fact that there were as many bunted heads in the plots of the first series as in corresponding plots of each division? Project 1.
So that, for example, $\frac{3}{5}$ of the heads of the fifth, twenty-ninth, and fifty-third, etc., were diseased and $\frac{7}{8}$ of the heads in plots 17, 41, 65, etc. I shall here call to mind what I stated in the beginning of my memoir on the manner in which these plots were sown.

On each date of seeding I worked on each of the 5 divisions and sowed regularly four plots. I used only 4 kinds of seed in each seeding and I sowed these 4 in each of the divisions. By this method, the seed sown in a given bed was the same each day for 5 others. Let us again cite an example: When I sowed the fifth plot I sowed also the twenty-ninth, the fifty-third, etc. It is now easy to see that the seed, being the same for each of these several plots, the end product had to be the same or similar and that the disease, once noticeable in one plot, must necessarily show up in the others in more or less equal amount. Any inequalities in the amount of bunt appearing in one plot as compared with another I attributed to differences in the quantity of inoculum that different kernels may have re-

ceived when I dusted them and to inherent differences in susceptibility to the virus.

It has been possible to note that plots 17, 41, 65, etc., devoted to beardless wheat, were almost wholly infected, and one would be tempted to believe that this kind of wheat is more subject to bunt than bearded wheat. But, among the experiments of 1752-53, I shall report one that seems to prove that this disease attacks bearded wheat with equal frequency and ease, or at least leaves one in doubt about the matter.

I stated that in the second, third, and fourth series of my plots I noticed some smutted heads. The seed sown in those plots, however, was selected and also treated for the second and fourth series. By what possible chance could those plots have developed any diseased plants?

To this I will first reply that in spite of the attention given to the choice of the seed it is almost impossible to escape the inclusion of some bunt balls, contact with which introduces an element of danger to the sound seed.

In the second place I will say that I made a mistake in my seeding in that I always began by sowing the smut-inoculated seed before I sowed the clean seed. It was also always the same hand that distributed the four kinds of seed and which, once soiled by the smut-inoculated seed, could contaminate other seed, even though prophylactically treated, as well as that that had not been so treated. Also I observed that the plots sown to the latter kind of seed bore more smutted heads than those sown to treated seed on which there remained a light residual coating of lime mixed with some particles of nitre and common salt. By the care I exercised in allowing an interval of several days between sowings I was able to do my planting at times when the conditions of the soil and of the atmosphere were the same.

On the 16th of October, date of the second seeding, the weather was mild and the mercury in the barometer stood at 27.9 inches.

The second seeding was done on the 22nd of the same month, following some consecutive rains. On that day there was a cold sharp wind; the barometer indicated an atmospheric pressure of 27.8 inches.

The third sowing date, October 27, was a very beautiful day; the barometer stood at 27.8 inches.

The weather on November 3, the date of the fourth sowing, was quite mild and partly cloudy. The barometer registered 27.3 inches.

On November 10, the date of the fifth sowing, a very cold wind prevailed. A light but nearly constant rain fell. The soil was heavy and sticky and hard to work. The barometric pressure rose to 27.7 inches.

The sixth and last sowing was made on November 22. The day was cold and very sharp; the surface of the ground was frozen; a fairly thick mist hid from view near-by objects; the barometer registered 28 inches.

I neglected to observe the thermometer during these different seeding operations, but one can approximate the temperature variation for the period by the air-temperature readings, which I have in general indicated.

With the exception of the 30 plots of the first series in which the smut developed so abundantly for the reason noted in the foregoing, none of the other plots, although 90 in number, produced any bunt, and that regardless of whether the weather was fair or foul at time of seeding. The plots of the sixth sowing, at which time there was a thick mist—for some farmers, such a bad sign—, were as exempt from the disease as those of the third sowing, done on a clear, calm day.

We shall not dwell longer, therefore, on purely assumed causes of an external character, such as rains and mists that prevail during the growth of the wheat crop; excessive or deficient soil moisture available to the seed and time of seeding. We cannot, I say, longer delay to consider all these things as being capable, in themselves, of causing smut. Even in cases where these conditions are unfavorable, it will always be necessary to assume the presence of a disease germ independent of such conditions and in the development of which they could, at the most, do no more than contribute.

There remains for me to say only a word concerning the moss plot the results of which have already been noted. The first division of this plot, in which I sowed seed blackened with smut, produced bunt in abundance. The three other divisions, in which I had sown selected and smut-free seed, or some of the same seed differently treated, were free from bunt, if one except the second division. But attention al-

ready has been called to the fact that proximity of the first division may have been responsible for this.

Project 1. Let us conclude then, without fear of error: (1) that, since the smut that appeared in the wheat grown in pure moss is just the same as that produced by wheat plants grown in soil, one cannot attribute this disease to the nature of the soil, to a particular kind of substratum.

(2) Since the smut was as abundant in the first division of the moss plot as it was in plots 21, 45, 69, etc. of the first series, it is necessary, in order to determine the cause, to take into account the seed, which was the same for the first division of the moss plot as that used in those particular plots. It is necessary to attribute it to the dust of the bunt-balls with which this seed was inoculated and which was as active in the moss as in the fertilized soil and the soil that had not been fertilized.

Finally, we may conclude that the smut could not have been due to excessive soil moisture, as Mr. Tull thinks. In the two last divisions of the moss plot not a single bunted head was found. Nevertheless, the wheat, in order to complete its different stages of growth, had to be continually irrigated. The moss, from which it derived its substance, was always soaked with water. It threw out numerous roots, which partook of the excessive humidity of the moss and which would have served only to render the disease more certain if excessive humidity had borne any causative relationship to it.

I might again attack in a more direct way the opinion of Mr. Tull. I have performed, I have, in fact, repeated, the experiment reported by M. Duhamel in his treatise on the subject of land culture. I observed but one wheat plant that produced bunted heads. This plant grew in soil allowed to remain continually impoverished. This experiment has been repeated and the details have been presented to the Academy for its judgment as to the weight one should give to the opinion of the English scholar.

I trust that I may be permitted before closing the consideration of my experiments of 1751-52 to make an observation that is, to be sure, somewhat apart from the subject here treated, but that I could not overlook because of its importance. It is based on those same experiments and is intended to convey the fact that a great deal of seed is

lost in the ground, that this loss is due principally to a too deep working of the soil, and that a little seed well distributed affords assurance of greater production than does much seed carelessly sown. In consideration of the light shed on this matter by Duhamel and several other zealous citizens, my reflections will seem, perhaps, out of order and certainly less conclusive than those on which they have depended and to which I feel indebted. My reflections are, however, based on experiments, and one cannot know too much to draw one's inferences.

The arpent of ground, in the country where I am contains 40,000 square feet. It is the practice among the farmers of our cantons to sow 5 bushels or nearly that amount of wheat per arpent. The bushel weighs from 35 to 36 pounds and contains 20 pints, the equivalent of 24 of those of Paris. In an average year the local arpent yield is 30 to 32 bushels. If we put the production at 24 it would be a fair average between the returns of the best harvests, say 40 bushels per arpent and those of the poorest, say 28 bushels.

I stated that the tract where I carried on my experiments was 540 feet long by 24 feet wide. Before doing anything else it is necessary to deduct from this width four feet, which served only as paths between the plots and, therefore, had no part in the production of the area. Five-hundred-and-forty multiplied by 20 gives 10,800 square feet of surface, the equivalent of 27 *cordes*, a *corde* being equal to 20 square feet. At the rate of 5 bushels per arpent an ordinary farmer would have sown on this area of 10,800 square feet one bushel and seven pints of seed; and on the basis of a return of 34 bushels per arpent he would have harvested 9 bushels and 3⅗ pints.

It will no doubt be recalled that my 120 plots, comprising 10,800 sq. ft., were 5 feet wide by 18 feet long and that the wheat was sown in rows, alternately, 5 or 6 rows per plot, according to the day of the seeding. In the first case, the rows were a foot apart and on the borders of the plot there was left a space six inches wide in order to allow to the first and fifth rows opportunity to grow without crowding the alleys. In the second case, where the rows were 6 in number, they were only 10 inches apart and the scant vacant space on each border amounted only to 5 inches.

I used for my 120 plots only 12 pints of seed or thereabouts. This amount of seed would seem excessive to those who carry on their experiments in accordance with the new practice, but I did not control the hand of the sower. The diseases of the crop, more noticeable in a number of plants, constituted my principal interest.

With the advent of spring, I was more than ever impressed with the necessity of a more careful handling of the seed; my plots being covered with verdure, the plants shot up vigorously. I was obliged to pull up part of the plants in those plots where the manure had been too generously spread. I was later provoked with myself for having failed to remove more plants than I did. The remaining plants, vigorous, erect, laden with heads, and more than ordinarily luxuriant had become objects of interest only to be blown down in several directions by a violent wind accompanied by rain. I propped them up with stakes, but in spite of my precautions, the grain suffered from the misfortune: it was shrunken and poorly filled; and, as a result there was a necessary depreciation in ultimate returns. It was, however, better than I had dared to hope, as the details here given will show. In the 30 plots where the smut was so abundant, this question does not enter into this first set of figures.

The first	part of the area fertilized with	pigeon droppings	yielded	2	bus.,	16	pints.
" second	" " "	sheep manure	"	2	"	19½	"
" third	" " "	night soil	"	2	"	16½	"
" fourth	" " "	horse and mule manure	"	2	"	11	"
" fifth	" not fertilized			2	"	14	"
		Total yield		13	"	17	"

The piece of ground treated with horse and mule manure produced a little less than that where no manure was applied, because it contained more lodged plants and it yielded a greater proportion of poor and shrivelled grain.

It is now necessary to add to these 13 bushels and 17 pints the yield of thirty plots that were smut-infested and of which I have, until now, taken no account. I did not thresh the sheaves produced by these invested plots. I kept them intact so as to remove from them the diseased heads and culms in order to provide inoculum for the experiments of 1752-53.

Since it was my responsibility that these 30 plots, constituting one fourth of the nursery, should be as exempt

from disease as the 30 other plots and as productive, the same amount of seed having been sown in those plots as in the healthy ones, their yield should be estimated as being one-third of 13 bushels, 17 pints, the total yield of the 90 healthy plots. I here present my figures:

Actual yield of the 30 healthy plots	13 bus., 17	pints.
⅓ this yield, the estimated return from the 30 infested plots	4 "	12⅓ "
Total	18 "	9⅓ "

I stated that an average farmer, in sowing a like area, would have employed 27 pints of wheat where I used but 12 and that, too, lavishly. Therein, already, was a saving of 15 pints.

This farmer, I said again, would have harvested but 9 bushels, 3⅗ pints of seed, while I would have obtained or would have been able to obtain 18 bushels, 9⅓ pints. My crop, then, returned to me a little more than double his yield. Thus 10,800 sq. ft., which, in any ordinary year, would have produced at the rate of 34 bushels per arpent, yielded for me, as a result of improved and crudely employed methods, a return of 68 bushels and 7⅗ pints per arpent.

At first glance one might perhaps attribute this marked improvement to the strength and abundance of my fertilizers; but, excepting the fourth division, so heavily fertilized as to be injurious, it has been already noted that the fifth division, where there was no manure, returned a yield of 2 bushels, 14 pints. If one add to this first amount the 18 pints from the 6 plots attacked by smut, this fifth division should have returned a yield of 3 bushels, 12 pints. Assuming a like return from the 4 other divisions, I should always have obtained from the 10,800 sq. ft. yielding 18 bushels, a very much greater return than that we have figured for the ordinary crop of a farmer working an area of the same size.

Continuation
of the Experiments and Reflections
relative to the same dissertation

The outcome of the different experiments I have presented seems sufficient to persuade me that the disease (bunt) was contagious, and that the virus was resident in the dust of the bunt balls. I scarcely hesitated so to conclude

after looking over that series of 30 infected plots alongside
1752, another series of plots where no smut at all appeared, in
1753 reflecting on that proportionate number of damaged heads produced by the plots of the first series, which, in each division were found similarly situated: a kind of equality or sameness that was difficult for me to attribute to any other cause than the condition of the more or less smut-contaminated seed; but always the same for the plots where the number of diseased heads was alike.

However, I was always doubtful as to the purity and intrinsic quality of the seed I had inoculated. I had not forgotten that it was in the main the poorest, the most defective I could find in the different sacks offered for sale, and I always feared that some imperfection peculiar to the seed and apart from the smut with which I had dusted it, was the real cause of the trouble.

My principal aim, then, was to determine whether a healthy, plump seed in perfect condition could produce bunted heads from those only that had been blackened with the dust from smut balls obtained from diseased heads. I felt, too, that I must assure myself as to whether the straw from bunted plants did or did not manifest the disease and if, once incorporated in the manures, it could be transmitted to any seed in proximity to it. I did more, I observed the principal disease of darnel grass, struck as I was by the agreement between it and that of wheat and tried to bring about the latter in the first-named plant by means of spores. I tried some experiments of this kind on rye, barley, oats, etc. and shall now relate all my operations.

They had for an object a goal of ultimate consequence. I, therefore, beg the enlightened company, to which I am under obligations to render an account, to permit me to present the minutest details and that I leave it ignorant of none of the particulars I have observed.

I shall follow exactly the order of my work. After having informed you regarding the preliminary arrangements incident to seeding, I shall say a word concerning the nursery where my experiments were carried on; I shall give their distribution; and shall tell of different kinds of seed and the different conditions, even, of the seed employed. I shall place each kind in the order followed in my sowings so that one will have at the outset a general prospect. I shall relate

in due course and in historical sequence certain observations I made at different stages in the growth of the wheat; I shall give the result of each particular experiment in presenting the reflections these observations provoked in me; and I shall finish by replying to three or four difficulties, which, although too trivial to cast doubt on the facts I shall here disclose, nevertheless merit one's seeking a solution.

The first thing to occupy me at the close of the year 1752 was the composition of the different manures I needed to use. On the 13th of September I had a Spanish horse lodged in a suitable stable. His litter consisted only of straws that had borne bunted heads. I shall refer to it in due course as *suspected straw*. It is known that straw is the ordinary fodder of this kind of horse. In the fear that the fodder fed to the horse, and of which a part fell about his feet, had not become mixed with his bedding, I was careful to feed him for 7 or 8 days, and not without a marked disgust on his part, nothing but bundles laden with considerable bunt. In that way the few bundles that had become mingled with the bedding could hardly have changed the condition of it. A little pile of the litter was drawn from under the horse and was put aside for use.

The stall was cleaned a second time. The same horse had for his bedding some straw the cleanliness of which I was certain; he was fed on more savory bundles than the first ones; also, he ate them with a relish. Within 8 or 10 days, I had the necessary amount of smut-free manure which I put in a pile for use, just as I did the first lot.

During the rest of September I got together the other kinds of manure that I had planned to use. I cut up in small bits the clean straw and mixed it with the ordinary proportion of the manures, as much with the sheep droppings, the horse manure, the sheep manure, the basis of which was hay, as with the cow dung. Each kind of prepared manure was put in a vat and watered several times to hasten rotting of the straw.

Whatever was done to prepare the manures of which the clean straw was a component part was done also in making up those into which the contaminated straw was introduced; the same fertilizer taken from the same heap to mix with the one and with the other; the same attention to the pro-

cedure; the same waterings; no difference anywhere other than that resulting from the straw.

Entertaining, as I did, the idea that the straw of smutted wheat plants might be dangerous for the wheat, I avoided walking on the ground that bore such plants so recently and that still contained the remains of stubble that I mistrusted. I chose a little field hedged in, with a good exposure, by a good seed bed that was to produce rye. Although the tract may have been one of the best in the canton, I spaded it under immediately after the harvest and ploughed it a month later. Some bushels of pigeon droppings were scattered over it, the only kind of non-suspected amendment that it was easy for me to find.

This field was 360 feet long by 74 feet wide. I did not use all of it but limited myself to a piece 360 feet long by 48 wide. The remaining portion was saved for part of the experiments of 1753-1754. I made there 120 plots, 18 feet long by 5 feet wide, laid out in six parallel series and separated by alleys, one foot wide. These 120 plots occupied a total width of only 36 feet by the whole length of the nursery tract. There remained, then, a space of 12 feet running the entire length of the tract. After having been sown, this was worked according to custom.

I followed the same method of seeding I had observed the preceding year. Each of my plots contained 5 rows, the rows were separated by alleys 1 foot wide, and on the 2 edges of each plot there was left a space of 6 inches in excess of the alley for a purpose already explained.

I stated that my 120 plots were laid out in 6 series, each series containing 20 plots, and the plots numbering 6 across the front. The first section for our consideration will be only the 100 plots contained in the first 5 series and designated by their numbers. The remaining 20 plots, which I have designated by letters, will require a thorough cleaning up because they have grown such things as darnel, rye, barley, and wheat, known under the name of mouse wheat [*Triticum cinericeum* G. Bauh.]. It is necessary, for the sake of clarity, that I treat this group of plots separately and consecutively.

Project 2.

Before entering into the details concerning each plot in particular, it is essential to make known the quality of the seed I employed. I withdrew it from several selected bundles

produced by one of the better cantons. I did not fail to remove from these bundles before they were thrashed some bunted heads found in them. The wheat obtained from the bundles was very choice; I noticed, however, 4 or 5 bunt balls that without doubt came out of partly bunted heads.

Triticum cinericeum G. Bauh. Lib. XVIII.

This selected seed was put into a special bag, and it was always from this one bag that I took as much pure and perfect seed for sowing as I took of that designed to become a source of bunt by becoming infested with this kind of dust. I shall designate the grain taken from this bag by the term *pure*.

I sowed in plot No. 1 some pure seed without fertilizer. This plot and all the others into the consideration of which manure does not specially enter are regarded as having received none. The pigeon droppings, spread on all the plots, exerted no influence on the disease in question and need not occupy us here.

Project 2.

The second and third plots were fertilized with noncontaminated straw thrown out from under the Spanish horse.

The fourth and fifth plots were fertilized with contaminated straw from under the same horse. The seed sown in these four plots was pure, and was the same for the other 16 plots that had been manured. In order that the seed might rest immediately on the manure, I had a little furrow made quite deep and the manure put in it. The grain was sown on it and then covered with soil. This operation was the same for the different manures.

Plots 6, 11, and 16, or, for that matter, all plots of the first series, were without special manure and were sown to pure seed only, or to another selected sort, with the exception of the 26th, where I applied some fragmented noncontaminated straw, and of the 76th, where I sowed wheat, contaminated after having washed it.

The 7th plot was again manured like the 2nd and 3rd, and the 8th like the 4th and 5th.

In the 9th plot, I placed manure consisting of sheep droppings and clean straw, and in the 10th, the mixture comprising sheep droppings and contaminated straw.

Plots 12 and 13 contained fertilizer consisting of horse manure and clean straw.

Plots 14 and 15 were manured with a mixture of horse manure and contaminated straw.

The 17th and 18th plots were fertilized with the manure consisting of sheep manure, the basis of which was a litter comprising only hay and clean straw.

The 19th and 20th plots were given the same sort of manure combined with contaminated straw.

The 22nd and 23rd contained manure composed of cow dung and clean straw.

The 24th and 25th, this same kind of dung with contaminated straw.

I have already stated that the 26th plot placed in the first series contained clean, chopped straw. In using this straw, I followed the same method employed in applying the other fertilizers: that is, I distributed it in a furrow dug to a depth of 4 or 5 inches, then sowed the seed on this straw and covered it.

(A) Glumae

I placed some contaminated straw in plots 27 and 28, some clean chaff (A), in 29, and some contaminated chaff in 30.

For plots 32 and 33 I set aside some clean wheat previously moistened with ordinary water in which I had melted some adhesive gum and then dusted it with bunt.

Plots 34 and 35 were sown with clean seed treated as in the two preceding plots but with this difference, namely, that instead of gum, I dissolved some fish glue in the water with which I dampened the seed.

Plots 37, 38, 39, and 40 deserve special consideration. The seed sown in the first two was clean wheat that had previously been heavily blackened with bunt, then moistened in this condition with ordinary water in which had been dissolved some common salt and lime. The seed thus treated lost none of the smut with which it had been dusted. After it was dampened, it seemed as though it had been dipped in very thick ink. The two last plots received clean wheat that had been prepared in nearly the same manner, except that the water with which the seed was moistened contained nitre.

Plots 42, 43, 44, and 45 I sowed with wheat that had been merely blackened with bunt. This seed was placed at different depths. In the first of these plots it was planted in the top soil; in the second at a depth of 1 or 2 inches; in the third, 3 or 4 inches; and in the fourth at a depth of 5 or 6 inches.

Plots 47, 48, 49, and 50 were destined also to receive seed at 4 different depths, which I shall indicate in the same order as has been noted. This seed consisted of noncontaminated wheat, but dusted with ground darnel seed. I have already stated that this plant was subject to the same disease as wheat.

I had a remnant of the horse manure with which only the contaminated straw had been mixed. I used it in plots 52 and 53 where I sowed clean wheat. I left plot 54 unmanured and sowed in it the same kind of seed employed in plots 52 and 53.

Sequel of the second project.

In drawing from my infected bundles the large quantity of necessary smut, I was careful to separate those heads that were partly smutted and partly healthy. These heads, from which I myself removed the sound, germinable grain, furnished me enough seed for one plot. This was plot 55.

In the 57th plot I sowed some clean wheat that I had blackened with smut and had left buried in this dust for more than a month.

I inoculated some clean wheat with dust from some [loose] smutted kernels of wheat, barley, and oats. I sowed these 3 lots of inoculated seed in plots 58, 59, and 60.

I selected from some badly infected bundles several healthy beardless heads and some that were bearded. I removed from the first lot the seed for plots 62 and 63 and from the other the seed sown in plots 64 and 65.

In plot 67, I sowed some clean seed but, before sowing it, I placed in the bottom of the furrow a trail of the dust from some bunt balls, onto which the clean seed fell and remained intact.

The 68th plot was prepared with greater attention to detail. After I had opened with the pick the customary little furrow in which to sow the seed, I placed in the bottom of this furrow a wooden rule 6 lines [½ inch] thick, which occupied the whole length of the furrow, and partitioned it into two parts. On one side of this rule, clean wheat was sown; on the other a strip of smut from bunt balls was laid down. The rule being lifted, the seed was found one-half inch, or nearly that, from the dust. It was then covered with soil, care being exercised not to displace it.

Plot 69 was sown with beardless wheat, blackened with bunt, and plot 70, with similarly inoculated bearded wheat.

Plot 72 contained clean wheat that had been previously treated with an aqueous solution of common salt and lime dust.

Plot 73 was sown with noncontaminated wheat previously pickled in an aqueous solution of lime and nitre; the 74th, with selected mouse wheat; the 75th, wheat of this sort, blackened with bunt.

I stated that plot 76 contained diseased but washed wheat. This seed had been furnished me by the Procurator of a Carthusian nunnery, alongside which institution is situated the field that I worked. The field of the Procurator produced much smut in 1752 and I perceived that certain advantages might result from using this seed under varying conditions. I used it in plots 77 and 78 after having treated it, according to my custom, with nitre or with sea salt and lime. In plots 79 and 80 I sowed it just as it came from the Procurator's granary. In the language of the farmer, it was the last.

Plot 82 contained selected spring wheat; plot 83 some of the same wheat dusted with bunt; and plot 84, some wheat similar to that I had at first moistened with an aqueous solution of mucilage and later dusted with bunt.

In plot 85 I sowed one seed at a time, 6 inches apart. I used miracle wheat, known to botanists as *Triticum spicâ multiplici*. This plot contained 5 rows, 3 of which comprised clean, nontreated seed; the remaining 2 rows contained seed with which had been mixed some seed of the same kind that I had dusted with smut.

The 12 plots, presently to be considered, were not sown until in March, 1753, but immediately following the others. Let us not interrupt the thread of our discourse.

I sowed some clean common barley in plot 87; some barley blackened with barley smut in plot 88; in plot 89, some barley dusted with wheat bunt; and in plot 90 some barley seed treated with nitre and lime after having been blackened with barley smut.

Plots 92, 93, 94, and 95 were set apart for spring wheat. Clean seed was sown in the first plot; clean seed also, with contaminated straw in the second; blackened with bunt spores in the third; similarly inoculated in the fourth but, before sowing, treated with nitre and lime.

Oats occupied plots 97, 98, 99, and 100. The seed was

sown under the same conditions as to treatment or absence of treatment as was that of the barley.

I now take up the 20 plots of the sixth series: In plot A, I sowed some healthy darnel seed free from all suspicion as to its purity. In plot B, I sowed some sound darnel seed that, prior to sowing, had first been treated with nitre and lime. Second Project

In plot C was sown disease-free darnel seed after having distributed in the furrows a large quantity of spores from the bunt balls of infected wheat. Plot D was devoted to healthy darnel seed dusted with bunt spores. Plot E, to healthy darnel inoculated with darnel smut; and plot F with healthy darnel inoculated with darnel smut, then washed.

In plot G was sown some darnel seed collected in a field of rye where there were found many other darnel plants attacked by smut.

The 4 plots marked H, I, K, and L were devoted to rye. Clean, nontreated seed was sown in plots H and I, seed inoculated with wheat-bunt spores was sown in plot K, and seed inoculated with the dust of some ergot grains I had reduced by grinding was sown in plot L.

Plot I differed from the others in that its seed furrows had been dusted with wheat bunt right where the rye kernels fell.

The 7 plots designated M, N, O, P, Q, R, and S were devoted to square barley (*Hordeum polistichum*). It was sown as clean seed in plots 1 and 2; (the five seed-rows of this second plot had been inoculated with wheat-bunt spores) inoculated with wheat bunt in the third and fourth; blackened with barley smut in the fifth and sixth; and the seventh plot was sown to wheat-bunt-inoculated seed, subsequently treated with nitre and lime. Continuation of the second project

I sowed mouse wheat in the two last plots. For the one marked T, the seed was dampened with mucilage water and dusted with bunt. The other, marked V, was sown with clean, nontreated seed.

It may be recalled that alongside the sixth series of plots I had reserved a space 12 feet wide and 360 feet long, the entire length of the area, and that I had divided this strip of ground crosswise into 3 equal parts. I sowed clean wheat in the first; some of the same kind of wheat in the

second but heavily inoculated with bunt and then allowed to stand a month before restoring it to its original condition by washing in several waters; and in the third plot I sowed clean wheat, which was first inoculated with bunt, allowed to stand for a month in this condition, and then washed superficially and again covered with a new coating of bunt to the extent of forming a sort of crust on the seed.

In this little tract of ground, of value to me because of what developed there, the seed was covered with the plow, according to custom. Before this was done, however, I took care to mark with a stake the furrow that separated each part and that had served to govern the seeding operations. I next had some barley (*Hordeum polistichum*) sown exactly opposite the stakes and thereby always had before me a visible object with which to mark off distinctly the 3 parts of the tract.

The rachitic condition is always the first disease to be observed in wheat. It shows in a very marked manner after the beginning of May. I had every opportunity in 1753 to make some observations on this peculiar condition, and I became more and more inclined to the opinion that insects contributed to it. The aborted or rachitic wheat plants are much more prevalent than one imagines. In the great number I have examined there were very few in which I did not discover staphylinids of every age, even on heads tightly enclosed within the boot and not more than half an inch long.

Three things principally have impressed me in observing rachitic wheat plants. I have noticed: 1. That they were ordinarily more advanced than the healthy plants, other things being equal, and that they produced heads sooner.

2. That the stem and the still-enclosed leaves of these diseased plants, on which are found some insects, are studded with some droplets of a very clear liquid, which seems to be exuded sap.

3. That their first leaves are ordinarily either withered or decomposed; that the part of the stem that is in the ground is often altered and shows a little of the decay shown by the first leaves.

Will the naturalists, accustomed to follow insects, find nothing in the phenomena of which I speak that resembles

those reactions often occasioned by these same insects in other plants?

The examination of the blighted plants occupied only a few days; I passed soon to a consideration of the bunted plants. I was so accustomed to distinguishing them from the bunt-free plants by their external characters that I very early recognized the disease in the special plots and in the third part of the plot worked by the plow, where the seed had been dusted and where I had already anticipated bunt would appear.

The great abundance of diseased plants I had at my disposal afforded me every facility to apprehend the malady in its beginnings and to follow its progress. I, nevertheless, renewed the observations I had made the preceding year on the bunted wheat. General stunting of the plant growth, high mortality among the plants, bluish color of stem and leaves, first hint of green in the youngest grains, their fetid odor when crushed, all agreed with what I have already said and would here constitute a needless repetition were I to describe them. I shall pass to the consideration of the different results obtained from my plots and from the little strip of ground alongside them.

I first collected my results while the plants were still *in situ*, waiting until after harvest to count scrupulously the healthy as well as the bunted heads produced by the plots. It will here be a matter of a kind of summary of the first estimate and of the exact calculation made after the harvest. It will be easy to judge of the particular product of each plot by glancing at the general plan of my operations and at the results recorded for each.

All of the first series of plots was excellent except for a few evident heads of bunt. Each plot, one with the other, bore 5 or 6 such heads. There were more of them in the seventy-sixth plot, as it had received a different sowing from that of the others and was under suspicion.

Second Project.

In general, there were more bunted heads, although in some instances but slightly more, in the plots prepared with manures containing contaminated straw than in those where the manures contained clean straw. The difference was especially noticeable in plots 17, 18, 19, and 20, where I had used two kinds of sheep manure. Plot 18, more affected

than 17, contained only 94 bunted heads in a total of 3,569,[1] while plot 19 contained 825 heads of bunt in a total of 3,325, and plot 20, 879 bunted for the total product of the plot, which was 3,257 heads. These two last plots had received the same seed as the first two but, at the same time, some contaminated straw had been placed in the seed row. One should hardly attribute this abundance of smut merely to the contaminated straw in these manures.

In considering the manured plots and especially those containing cow dung, I at first thought that the straw, of whatever sort, offered no element of danger to the seed; but plots 19 and 20 made me apprehensive, and it soon will be seen that I had cause for suspicion, for, when such straw contained some diseased heads, it contained a virus that could not be too much feared.

It would be marvelous if the contaminated straw, once buried in the manures, lost all its banefulness.

According to every indication it did not lose its harmfulness in the mixture I made of it with sheep manure. But I should note here that this manure was very dry when I used it, that it was easily reduced to a dust, that it contained much hay and, therefore, may have lacked sufficient action and heat to reduce to a certain state of decay the contaminated straw with which I had mixed it. This year I performed again the experiment involving use of the different manures. That part of the experiment relating especially to sheep manure mixed with contaminated straw will command my attention.

The wheat of plots 27, 28, 29, and 30 failed to resist the rigors of winter. Its roots, interlaced in the straw and not directly covered by the soil, were frozen nearly their whole length. This loss I felt. I also experienced a little damage in two other plots of the same kind, to be considered in their turn.

Plots 32, 33, 34, and 35, purposely sown to bear smut, yielded a sufficiently large quantity of it but less than I had

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[1] One may regard this account, as well as those accounts that will follow, as done with all possible care. I have strongly felt that one might hesitate to accept the results I would have to give for several plots if I produced only simple estimates. I am, therefore, led to the point of culling from a large number of bundles healthy and diseased plants, healthy heads along with diseased ones, counting the one and the other and of giving the same attention to the plants bearing healthy and diseased heads as to heads bearing both healthy and diseased kernels.

predicted. Perhaps the mucilage and fish glue that entered into the treatment of the seed formed on its pericarp a light glazing, which arrested in part the action of the dust with which the seed had been inoculated.

The virtue of sea salt and saltpeter, especially in combination with pulverized lime, in preventing the effect of the bunt dust on the healthy seed is clearly shown by the condition of plots 37, 38, 39, and 40. They were very luxuriant; I could find scarcely no diseased heads in them. They were in the midst of the contagion. The 4 plots that preceded them and those that followed bore an abundance of smutty heads. Moreover, even seed taken from the same sack for these 12 plots and inoculated with the same spore dust in equal dosage was, in part, unaffected by the inoculum. What is the conclusion? That the remedy appeared to be effective.

In sowing some smut-inoculated seed at different depths in plots 42, 43, 44, and 45, my purpose was to examine (1) whether the wheat, sown at a depth of 6 inches and before it had thrown out several roots prior to emerging above ground, could, by virtue of its many roots, acquire a vigor that would render it less liable to disease attack and diminish the effect of it. But the virus, once incorporated in the germ of the wheat seed, is too active to be arrested by any strong vegetative growth. In plot 45, where the seed had been sown 6 inches deep, I had only 70 healthy wheat plants, bearing 331 heads; while, in the same plot, I found 211 diseased plants, bearing 938 heads of bunt. Several plants, however, were very vigorous; one, among others, producing as many as 27 culms bearing heads measuring 7 to 8 inches in length. Three-fourths of the plants in this plot were, therefore, attacked by the disease and, although they were strong plants, they were not proof against the virus. I should, nevertheless, state by way of qualification of what I have said that a large number of these sick plants bore heads some of which were healthy and some were bunted.

The second objective I had in mind in sowing the seed at different depths was, in fact, to prove that wheat, although grown from shallow-sown seed, may survive with vigor and in excellent condition, the rigors of a very cold winter, and that it perishes in great part if the seed be

planted too deep. This same plot 45, of which the seed rows were 5 to 6 inches deep, bore only 281 plants and 1,269 heads, although it had received the same quantity of seed as was sown in plot 42, where the seed was sown in the topmost layer of soil and the yield was 1,057 plants, bearing 3,713 heads.

The seed of plots 47, 48, 49, and 50 had been inoculated with dust from bunted seeds of darnel. This dust was almost as disastrous to wheat as was that of the wheat-bunt balls to the 4 preceding plots. This fact was astonishing; but 4 plots, where the phenomenon was equally noticeable, seemed to enable me to make certain of it. It will appear very singular, moreover, that this darnel bunt, which is quite similar in its color to its Spanish relative, left no hint of it in the seeds attacked by it. Its seeds, in fact, though ruined solely by the virus that this dust conveys to the seed, which in turn produces it, inevitably become changed to a black dust and differ in no respect from those that become bunted in any other way.

The conclusions I have drawn in speaking of the 4 previous plots must bear some relation to these latter ones. Vigor of plant growth is incapable of arresting the action of the virus. A great quantity of the seed perished solely because it was planted too deep. We have seen in the account of plot 45 that nearly ¾ of the seed planted there perished. The loss was not so great in plot 50, however, where the seed furrows were 5 or 6 inches deep, as it was in plot 45. Plot 50 bore 662 plants and 1,901 heads, 998 of which were bunted; while plot 48, which was of the same order, comparable with plot 50 and in which the seed had been sown at a depth of 2 inches, bore 1,143 plants and 3,092 heads 898 of which were bunted.

Nearly ¾ of the seed lost in plot 45, because of the excessive depth of the furrow, nearly half smothered also in plot 50 for the same reason, left no doubt that a considerable quantity of seed was lost in resistant or too firm soil and that the recommendation of the new culture is one of those most widely conceived.

Continuation of the second project.

The results of plots 52 and 53, where I had applied horse manure mixed with contaminated straw, were quite the same as those for the other plots where this kind of manure had been employed; that is to say, the bunt heads were few

in number but fewer still, however, than in the plot where only clean wheat had been sown as, for example, in plot 54.

The healthy grains of wheat, which had been picked out of a partly smutted, partly sound head, apparently contained no dangerous leaven; at least, it is true that plot 55, where I had sown only this kind of seed rescued from contagion, bore only bunted heads.

It would seem from the condition of plot 57 that the wheat that had remained covered for some time by the smut dust from the bunt balls, before being put in the ground, acquired no more disposition to produce smut than when sown immediately after being inoculated. In this plot only ⅖, or thereabouts, of the heads were affected. The damage is sometimes greater, although one may have taken less pains to occasion it.

I should say, to be sure, that this plot was poor in stand and quality, although the seed sown there had not been more sparingly used than in the other plots. Perhaps the cause, whatever one may suppose it to have been, for the bad condition of this plot and of the loss that obtained for a considerable part of the seed, resided chiefly in the kernels that were most infested with bunt and had only a slight effect on those into which the virus had penetrated less deeply.

I was much surprised in examining plots 58, 59, and 60 to find in them, especially in plot 58, more heads of bunt than I would have believed present. The seed sown in the 3 plots was from clean wheat that I had dusted with the spores from smutted heads of either wheat, barley, or oats. The loose smut, the dust, which is a result of it, has it also some contagious malignity? I am not yet able to persuade myself. Loose smut is a disease sometimes combined with bunt. Some bunt balls were found in heads of loose smut. These I used in inoculating the seed of plot 58. It would then have been less astonishing had I found in it a larger quantity of diseased heads than I anticipated.

Plots 62, 63, 64, and 65 contained but few bunted heads; enough, however, to cause me to get the workmen to abstrain from using the kind of seed I used. These bunted heads had been culled from several heads, as many bearded as beardless, drawn out of bundles containing an abundance of bunt.

The dust from the bunt balls that I had spread in the bottom of the seed furrow of plot 67 was completely effective. The seed that lay upon it soon contracted the poison and gave rise to a large number of bunted heads.

The disease was not less manifest in plot 68, though the seed, carefully placed in the furrows, was put a little way from the train of spores I had sown there and none of the kernels could touch it except through the roots they might push out. The seed I sowed in this plot was that same clean wheat, which, used alone in 30 places, gave, so to speak, no smut. We must, therefore, conclude that, if it occurred in this plot, it was because even the roots, yet hardly in their infancy, were susceptible to the virus contained in the smut, for, in this case, it was only through their intervention that the seed could receive and transmit it.

Plot 69 bore 3,117 beardless heads of which number 1,476 were bunted. Plot 70 produced 3,329 bearded heads, 1,705 of which were affected by bunt. The seed used in these two plots had been equally heavily inoculated. It would seem that one would be justified in concluding, against the opinion of some writers, that bearded wheat is more subject to bunt than is beardless wheat, but a single experiment does not warrant a decision; it simply serves to cast some doubt on any advantage that might follow the sowing of the one in preference to the other.

Although the seed employed in plots 72 and 73 was in no wise under suspicion, I treated it with a combination of lime and saltpeter, or lime and sea salt. I am happy to state that I observed at the time not a single head of bunt in those 2 plots. Four or 5 were, however, subsequently found there, and I could not see at all that the remedy had been of any advantage. From the same seed, used several times without treatment there developed, also, a few heads of bunt.

The mouse wheat, which occupied plot 74, gave me the satisfaction not afforded by the 2 preceding ones. I observed in it no heads of bunt; even in plot 75 there was no great number of them where the same seed of this variety had been dusted with smut prior to being sown. This kind of wheat seemed to me less subject to bunt than the others; but, on the other hand, the rachitic condition was quite common in this variety.

The contaminated wheat of the Carthusians, sown in plots 76, 77, 78, 79, and 80, responded, so far as concerned healthy or diseased heads, wholly as I had expected. Plots 79 and 80, where I had sown it in its imperfect condition, bore a large number of bunted heads. Plot 76, where the seed had been washed before sowing, contained much less smut than the other two. Plots 77 and 78 were excellent; bunted heads were very rare. The seed of these two plots also had been treated with lime and saltpeter or with a combination of lime and sea salt. As one may easily observe, from the little bit of smut sometimes left on the brush end of the seed, sometimes nearly all removed by lotions, sometimes inhibited by the action of seed treatment, there resulted some marked differences in the 5 plots in which the wheat of the Carthusians had been sown.

Spring wheat is very subject to smut. I therefore sowed some winter wheat in plots 82, 83, and 84, my idea being that the result might prove better with this kind of wheat. My conjectures were poorly founded. Plot 82, it is true, produced only a very few diseased heads, but the seed sown in this plot was clean. The 2 other plots, the seed of which had been inoculated before being sown, produced smut in abundance: half of the heads were ruined. These 3 plots gave a thin stand; I presume, therefore, that spring wheat is not so resistant to severe freezes as is winter wheat.

The Miracle wheat, sown in plot 85, produced no bunt heads, although I had inoculated much of the seed sown there. I observed not the slightest trace of the disease in another plot where I had sown the same kind of wheat, taking the precaution to inoculate some of the seed. Nevertheless, I would not yet dare feel too confident that Miracle wheat is not subject to bunt.

I am better informed as to common barley. It seems to me certain that it is not at all subject to this disease, even when the seed is smutted with the bunt of wheat; but, on the other hand, it is very subject to smut. There were, however, few heads of smut in the 4 plots where I had sown the barley: they were limited to 6 in plots 87 and 89; to 39 in plot 88 and 35 in plot 90. Would, therefore, the dust of the smutted kernels constitute an element of danger for the seed inoculated with it? Would saltpeter and lime have any effect on it? Plots 88 and 90 would lead one to suspect it.

Plots 92, 93, 94, and 95 were devoted to spring wheat. Clean seed was sown in the first; consequently, it produced only 8 diseased plants, bearing 15 bunted heads, while in the same plot there were 1,484 healthy plants, bearing 3,124 sound heads. Such was not the case in the second of these plots. The same spring wheat, out of the same sack (and this is worthy of note) had been sown in this plot, clean and copiously, from fear that part of it might die, on the chopped winter-wheat straw and absolutely similar to that used as contaminated straw in the composition of the different manures. This spring wheat, clean as it was, produced 676 diseased plants bearing 1,165 bunted heads and 2,025 healthy plants, bearing 3,215 sound heads. Thus the disease was present in more than one-fourth of the total number of heads. Could one attribute to anything other than to the contaminated straw this large amount of smut? It was still more abundant in plot 94 and that, because the seed, prior to sowing, had been inoculated with bunt. It was not, on the contrary, manifest in plot 95, except in very small quantity, because the seed, after it had been inoculated as in plot 94, was treated with saltpeter and lime. In addition to the fact that plot 95 was remarkable for the small number of bunted heads, it was also, by virtue of the vigor of the plants and length of culms, taller by 7 to 8 inches than the stand in plot 94. The treatment produced a double effect: it protected the wheat against the contagious disease and favored its complete growth.

The condition of the four plots that we shall consider forces one to reflect on the danger resident in the use of contaminated straw and on the existence of a contagious poison in the dust from the bunt balls and on the success of the remedy employed to prevent the effect of the poison. Spring wheat seemed to me to be more subject to loose smut than did winter wheat.

Bunt attacks oats no more than it does barley. This plant shows no symptom of the disease through any method yet employed to communicate it. Plots 97, 98, 99, and 100, sown to oats, were very luxuriant, a good stand and free from trouble. I observed in each only 4 or 5 heads of smut. The spores of smutty oats with which I had blackened the seed of plots 98 and 100 produced no effect.

I was able to save but few plants in the majority of the

remaining plots. They were sown on the 7th of November, 1752, at a time when the soil was extremely dry. The darnel grass, the rye, and the barley in those plots remained in the soil a long time before germinating, winter overtaking them in an incipient stage. The frosts were severe; nearly all the plants died. This little setback, however, but served to instruct me in the necessity of sowing rye and barley early to avoid its being injured by the cold while it is yet delicate and meagerly supplied with roots.

In plot A, where I had sown healthy darnel seed, I had no bunted heads among 141 plants I had collected. Of the 240 heads of darnel obtained from plot B, I found none affected with bunt. The seed sown in this plot had been inoculated with spores from smutted heads of darnel and then treated with saltpeter and lime.

Moreover, I did not observe any smutted heads of darnel in plot C in which the seed-furrow bottoms had been dusted with bunt of wheat prior to sowing the seed in them. This plot contained only 135 plants.

The darnel seed sown in plot D had been inoculated with spores from the bunt balls of smutted wheat. In spite of this, however, of the 230 heads produced by this plot not a single diseased head was found among them.

The scene now changes. The seed of plot E had been dusted with the spores of smutted darnel grains. Of the 320 plants produced by this plot, there were 224 healthy ones and 96 stricken with smut.

The seed of plot F was first inoculated like that of plot E, but washed some time after and sown in that condition. Of the 346 plants found left in this plot, there were 217 sound ones and 129 the heads of which were smutted.

I had sown in plot G healthy darnel seed taken from a bundle in which there was a large number of smutted heads. Nothing untoward resulted from this so far as concerned the smut; for, among the 464 plants obtained from this plot, not a single smutted head was found.

From what is to be observed in the product of these 7 plots we conclude (1) and certainly, as will be seen from the account of another plot, that the smut peculiar to darnel is contagious for darnel just as we already have noticed it to be pathogenic for wheat.

(2) That when the darnel seeds have once been inocu-

lated with smut from the same kind of grass the solutions may remove the larger portions of this contagious dust but not eliminate a certain malignity that it has conveyed to the seed.

(3) That there is some indication (I am yet sure of nothing) that the bunt of wheat is innocuous on darnel.

(4) And, finally, that the remedy of which I made use is one of the best, since plot B, where it was applied to the smut-infested seeds of darnel, contained not a single diseased head in the 240 heads it produced.

In the few plants I obtained from plots H, I, K, and L, I observed that the bunt of wheat had no effect on rye; also, that this disease never attacked it. The one with which rye is commonly stricken is ergot. I observed several ergotized heads among those produced by plots I, K, and L, but not enough of them in plot L to lead one to suspect that the dust from ergotized grains was endowed with anything of a contagious nature.

I believe it is now more firmly than ever established that ergot is the result of the sting or puncture of an insect. In examining a large quantity of ergot grains from rye I noticed that several contained a worm, hardly visible to the unaided eye, which, without doubt, had originated and fed there. I enclosed 20 such worm-infested ergot grains in a glass goblet covered with parchment. There the little worms lived, grew, and nearly consumed the ergots. If I can follow them through their metamorphosis, I shall be able to recognize from their appearance the insect that has produced them and that should be the same as the one that deposited the eggs in the ergot within which they were hatched.

Four-row barley certainly is not subject to bunt. I have every reason to believe it is not more so to loose smut. Among a considerable quantity of heads obtained from the 7 plots marked M, N, O, P, Q, R, and S, I found none in which I could observe the slightest vestige of one or the other disease.

I have already stated that mouse wheat is less subject to bunt than ordinary wheat. The results obtained from plots T and V confirm this assertion. In the first was found only a moderate quantity of bunted heads, although the seed sown in them had been abundantly dusted with smut. In the second plot, sown to clean seed, it was with difficulty

that I found one or two diseased heads. The stunted condition was noticeable here, as in all the other plots devoted to the same kind of wheat.

The little tract of plowed ground the laying out of which has already been noted, alone gave me, on the point I sought to establish, such a degree of assurance as would have sufficed in a less interesting investigation. The first part of this plot was notable for the number and height of the stems, for the excellence and healthy condition of the heads. It was only after a long search that I was able to find some bunted heads.

The second part, fairly good but less uniform than the first, contained a large number of bunted heads.

As for the third, it was in a lamentable condition, but essentially satisfactory to me. A thin poor stand in several places, it bore only weak, stunted, bluish plants, infected throughout with a prodigious amount of bunt, it yielded up a very disagreeable odor and led me to suspect thenceforth that it might be baneful to the nascent kernel stricken with it.

The seed in these three different parts of the nursery was precisely the same and came out of the same sack. The healthy, vigorous condition of the first plot indicates, therefore, the advantage of using none but healthy, well-selected seed, even without any previous treatment. The unsatisfactory condition of the second part warns one of the risk run by sowing contaminated grain, even after washing it several times. The diseased condition of almost all of the third plot, therefore, proves convincingly that the dust from the bunt balls of wheat contains a virus, a hidden malignity, that passes into the most healthy plant and intimately permeates it.

The tract, 540 feet long by 24 feet wide, where the experiments of 1751 had been carried on, served me for two others in 1752. Project 1.

The first series of plots, almost entirely infected with bunt, furnished me a favorable occasion to make a demonstration the object of which was of some practical value. In this first series, the fifth plot of each division was one of those where the disease was most manifest. There I pulled up by the roots the stubble remaining after the plants were cut. This same operation obtained in the 6 plots of the

division. Immediately after, the entire series was worked and the stubble was buried in the 20 plots where I had left it. This series of 30 plots, which formed no more than a neck of land, was sown to selected but nontreated seed.

It is well known that my object here was to observe whether, in a field that had produced much smut, a large quantity of stubble left there unrotted would constitute a source of danger to seed sown in such a field immediately after harvest. Fortunately, it turned out, by an exact comparison of places where the stubble had been taken away with those where it had been allowed to remain, that in spite of every suspicion at first entertained, the stubble was not an element of danger to the seed. I had very little smut in the entire series; and the 4 first divisions contained as little of it as did the fifth. In the general conclusion will be seen the deduction I have drawn from this experiment by combining it with others of the same kind.

The remainder of the tract, comprising 3 other series, was devoted to spring wheat. I conformed to the customary practice as to date and manner of sowing with one difference, namely, this: From the lot of seed designated for sowing this tract I kept out enough to sow an area 40 ft. x 18 ft. This area, as is easily seen, was taken from the same tract of which I am speaking. I inoculated some winter wheat to sow in this small area. The entire tract was seeded the same day and both kinds of seed were covered to the same depth with the plow.

From the month of April one could distinguish from a distance the little piece where the inoculated seed had been sown. The heads of wheat were somewhat poorer than those elsewhere in the tract; they were of a yellowish cast, and I did not know why they did not thrive. In due course they assumed that bluish color characteristic of bunted heads and matured by bearing almost nothing but heads attacked by smut. The rest of the tract, on the contrary, showed but few indications of the disease; I could scarcely find a single bunted head. This piece was crossed by a footpath and the little piece of smutty wheat was very close to this path. Many people had occasion to consider it; and they were surprised to see the disease thus concentrated within a limited area.

The tract as a whole produced a large number of heads

of loose smut: the spring wheat, it seemed to me, was more subject to it, but I hesitate to assert that it was also more subject to bunt than were the several varieties of winter wheat. M. Duhamel is not of the same opinion; and that was enough to make me hesitate. However, it must be said that the particular variety of spring wheat that I employed, consistently produced a prodigious quantity of smut [bunt] every time I wished to make it do so.

Continuation of Experiments, p. 114.

In reviewing the work of M. Duhamel, which I cite here, I am of the opinion that he did not agree perfectly with certain facts I have already disclosed; and I felt that it would be useful to narrate here, for the sake of clarity just what this scholarly academician has overlooked.

"There are," said M. Duhamel, "many heads the kernels of which were filled with a black dust instead of white flour. It is a disease common to several kinds of grain; it is called blast or *smut*, and the grains attacked by it are said to be blasted or smutted."

"Wheat, spring wheat, winter barley, barley, oats and, I believe, darnel, as well as several kinds of couch grass, are subject to these diseases, but certain grains more generally than others. Spring wheat, for example, is less susceptible than winter wheat."

"I have never seen smutted rye."

M. Duhamel designates by the term *blast* the disease I have characterized by the term *smut* [*loose smut*] and applies the name *smut* to the disease I designate as *bunt*. I shall, for the moment, employ the expressions used by M. Duhamel, for it will serve to clarify a part of his publication where they are used.

Wheat, both winter and spring, is unquestionably subject to loose smut and bunt.

I have every reason to doubt that 4-row winter barley is subject to loose smut of wheat and I am certain that it never becomes afflicted with bunt.

Barley and oats are subject only to loose smut.

I have not seen darnel grass affected by loose smut but this plant is very subject to bunt. Up to now, or I greatly deceive myself, this latter disease had not been recognized in darnel grass.

Rye is subject neither to loose smut not bunt.

In general, the first of these diseases is much more com-

mon than the second. In addition to the plants under consideration, among which it is very common, I have noticed it on 4 (a) others the first 2 of which are perennials and the other 2 annuals.

(a) *Gramen avenaceum*, elatius, jubâ longâ splendente. Raji synopsis. 260

Gramen arundinaceum, acerosâ glumâ, nostras. Park Theat. 1723.

Bromus panicula patente, spiculis ovatis. *Linn. flor.* Succ. 84.

Milium vulgare.

In the main, the experiments I have described present something very striking. They will certainly carry conviction when I shall have compared them with other experiments conducted on a small scale and over which I had every opportunity to watch, and with which they agreed perfectly.

The same garden surrounded by buildings, concerning which I have already spoken in referring to some tests I made in 1751, served my needs for the operations of 1752. This garden, somewhat irregular in shape, consists of two squares.

In the first square I laid out 2 plots, each 28 feet long by 5½ feet wide. I sowed in each plot 8 rows of wheat, one seed at a time, and 3 inches apart in the row. The first

Project 3. 4 rows contained clean seed only and in the 4 remaining ones were sown only wheat that had been inoculated with smut.

The first 2 rows of plot B were of clean, smut-free wheat. The third and fourth rows contained similarly clean wheat, but moistened with dilute mucilage and then dusted with bunt. The fifth row contained seed that I had picked from heads of wheat that were partly smutted and partly disease-free. The sixth was sown to some contaminated seed from the monastery, which I had washed. The seventh and eighth rows contained some of this same contaminated seed, but unwashed.

I shall not enter into the details of the product of these

Project 4 2 plots. You will be able to get these at a glance from the 2 plot diagrams at the close of the text.

The second square of the garden contained 5 plots, 15 feet long by 3 feet wide. In each of the plots marked C, D, E, and F there were 4 rows. The first 2 of plot C contained

clean seed and the last 2, the same kind of seed but inoculated with smut.

The first 2 rows of plot D were sown to clean seed that I had first smutted and then washed. The remaining 2 rows contained seed of the same kind, inoculated after having been moistened.

The clean wheat, which comprised the 4 rows of plot E, had been sown as follows: 2 rows on a mixture of horse manure and clean straw; and the remaining 2 on manure of the same kind mixed with contaminated straw.

Some disease-free darnel seed was sown in the first 2 rows of plot F and some seed inoculated with bunt dust peculiar to the species was sown in the remaining two.

The location of plot G did not permit me to make any rows lengthwise, as in the other plots; There were virtually 13 of them crosswise of the plot. The first 6 were sown to clean seed on clean straw; the 7th to darnel seed inoculated with the dust of darnel smut; and the last 6 to clean wheat on contaminated straw.

Again, one will easily perceive at a glance the result of these 5 plots alongside the plot divided into 2 squares. I come now to some observations that, as well as 2 others, I have had opportunity to make.

In spacing the seeds in the row one does not know which plant will be endowed with the greatest vigor and throw out the greatest number of tillers. This vigor, as we have already remarked, is incapable of arresting the action of the virus, once it becomes incorporated within the plant. I present here new evidence in support of this fact.

In a total of 445 wheat plants produced in the 4 rows of plot A, 333 of them were attacked by bunt. The vegetative growth in these 4 rows was, however, quite rank, considering the location of the plot, and the scant amount of air available to it. Each plant produced 4 to 5 culms. Project 3.

In washing the contaminated seed before sowing, a real benefit obtains; but this salutary treatment is not sufficient to protect the resulting plants from the disease. The seventh and eighth rows of plot B, sown to contaminated wheat, produced in one row 32 diseased and 72 healthy plants and in the other, 49 diseased and 67 healthy plants. The sixth row, in which was sown some of the same contaminated seed that, before sowing, had been washed, pro-

duced only 7 diseased plants in a total of 108. There was certainly a marked advantage obtained solely through the lotions; yet, 1/15 of the crop was lost.

Project 4. In plot C, 2 rows of clean wheat were separated from the 2 inoculated rows by a space of only 7 or 8 inches. This proximity of the 2 sets of rows afforded me the advantage of following the progress of the healthy and diseased plants and to compare their respective conditions. When the plants had attained a height of 4 or 5 inches I began to notice a certain abnormal aspect in the plants of the 2 last rows. The leaves of the ailing plants in due course took on a bluish cast, but remained always narrower by 3 or 4 lines than those of the healthy wheat. The inequality in height of the culms in the 2 sets of rows was maintained throughout; it seemed as though those of the last 2 rows had been cut so as to permit the healthy rows to dominate in height by as much as 8 inches.

So large a number of bunted heads as 933 out of 1,401, the total of the first 2 rows of plot D, is still further proof that otherwise very sound seed wheat that has been allowed to stand a long time in a mass of bunt dust contracts a poison that in great measure prevails even after the seed has been washed in ordinary water.

It is seen from the results of plot E that the suspected straw was harmful to the seed. The 2 rows where this straw was incorporated in the manure, contained 435 bunted heads, while the other 2, where the manure had a basis of clean straw, contained only 244.

Probably nobody will longer doubt the malign effect produced on darnel by the smut peculiar to it when he sees the result in plot F. The darnel seed I employed in these 4 rows came from the same little sack. On the one side, it produced scarcely any but sound, healthy plants; on the other, it caused a quite general disaster.

I have already stated that the bunt of darnel has all the external qualities peculiar to the same disease in wheat. Insidious destruction of the stamens; yellowish color of the tips of the ovaries; floral failure; extraordinary enlargement of the nascent fruits; their greenish tint when they begin to break down; fetid odor when crushed—all similar to what obtains in wheat, except for this difference, namely, that in darnel the farinaceous substance of the seed, which should

become smutty, takes on an ashy white color and finishes by changing into a dust quite like that of Spanish bran. The floury substance of the bunted wheat grain, on the other hand, assumes a blackish tint on the slightest approach of the disease, and retains this color while changing to a dust. I have not, at all, perceived, in examining those 4 rows of darnel several times, that there was, as in the case of plot C, any difference between the first pair and the last, either in the height of the stems or in the color and width of the leaves. It would be very astonishing if, in a darnel plant, the seeds alone suffered from a virus that reached them only by way of the canal of the plant, without the plant itself bearing some external signs of its effect.

The result given by plot G merits some attention. The first 6 rows produced only 11 bunted heads out of a total yield of 783, while the last 6 produced 122 out of 942, their entire yield.

The result obtained from plot 93 has led us to suspect with sufficient reason that the contaminated straw contained something harmful to the seed. Have we not had opportunity here to substantiate this deduction? Is it not striking that there was not a single one of those 6 last rows that did not bear more bunt heads than all of the 6 first ones, together?

Continuation of the Project 2.

By the beginning of September, 1752, I had need of a considerable quantity of contaminated straw for making up the fertilizers. After I had gleaned the stems, laden with their infected heads, from a large number of infected sheaves, I tied them into little bundles and threshed them in a barrel in order to get out the bunt balls. I took pains to remove all of them from the heads. In order that none of the dust remain attached, these heads were beaten with a beetle and rubbed vigorously with the hands.

Those who have seen bundles threshed in which bunt is abundant and have seen the grain from them winnowed, know how much the dust from the bunt balls, broken by the flails, spreads in the barn and what quantities of it become attached both to the wood and to the straw.

The barrel where my bundles of smut were threshed, was by chance very near the second square, next to plot F, quite far from plot G, sheltered, moreover, by a building and some distance from the first square.

It is not astonishing that the abundant dust that came out of the barrel was distributed to the nearest soil and that it became the cause of disease for some of the seed sown there. Plot F was not subject to the effect of the spread of the dust. We have seen that it was not dangerous to darnel; but plot E, farther from the barrel than any of the others, was also the most diseased. Plot D, designed to bear smut throughout, need not be considered here. The first pair of rows in plot C showed fewer signs of the disease than did the corresponding pair in plot E, the farthest from the barrel; and, for the same reason, plot G, as well as those of the first square, bore much less smut than all the others.

One will be even more inclined to believe, after I shall have reported on the following experiment, that the dust from the bunt balls, spread out over some of my plots before they were sown, and became there the source of infection of a large number of heads.

Perhaps I should pass in silence and be content to enjoy in secret the instruction it has afforded me. It would disclose in me a trait of contemplated malice and make me appear a dangerous neighbor. But it is to the public interest. Moreover, I am prompted only by ideas of helpfulness, even though I make mistakes. The damage I occasioned was not very great; moreover, it befell people who are no longer concerned with things here below.

The Reverend Carthusian Fathers have a piece of land at their door and hard by the little field I am cultivating. The day I sowed plots 67 and 68 and when, as a consequence, I had in my possession a large quantity of the dust from the bunt balls of diseased wheat to distribute in the seed furrows, the Reverend Fathers were sowing some wheat in their field. My work finished, I had a little of this dust left. I profited by the favorable occasion which presented itself, by using this dust in another tract than mine and thereby made a duplicate test.

On the evening when the workmen of the Carthusian Fathers had unhitched, I entered their field; I observed that part of the ground was yet unsown and that, according to all appearances, it would be sown on the morrow. I spread a little of the remaining bunt dust in an easily observable spot in this part not yet sown. The space within which the dust fell was probably 8 or 10 feet square. On the morrow, as I

had thought they would, they sowed in the rest of the field some wheat simply dipped in limewater and the whole lot was worked in to a considerable depth.

I lost sight of this experiment until the month of June, 1753, when I went to examine the plot belonging to the Reverend Fathers and recognized the little tract I had inoculated.

When I was about the put my measures to the test of finding it, it unquestionably made itself known by the bad condition of its crop of heads. The majority were attacked by smut; while, in the rest of the piece I could perceive almost no bunted heads. Brother Portier of the Carthusians had frequent occasion to examine this piece of wheat; it was under his very eyes. I called to his attention the little part infected with smut; he was struck by the great number of diseased heads presented to his view by such a small area and even more surprised to see it in this place. The cause of this phenomenon seemed, nevertheless, quite natural to the good Brother: an evil wind or the mist, said he positively, produced that spot. I sought not to disabuse him; perhaps I would have been unsuccessful had I attempted it.

The results of the large number of experiments the details of which have been given, afford, I believe the strongest evidence of the fact that I had set out to determine. I shall not, therefore, report the tests I made again with a view to confirming it, either the one in which I employed pure moss, as in the preceding year, or the one in which differently prepared soils were put in boxes and pots and sown to the same kind of seed or the same depth of soil, different seed according to the conditions under which I sowed it, or by putting in rows of isolated plants wherever my consignment of ground permitted. I was now wearied at seeing the surprising effect of some particles of dust and of always seeing the same result in a thousand places.

It will suffice here to assure you that the smallest tests have always agreed more or less perfectly with the experiments carried out on a large scale and that the smut disease, always contagious through its dust, has never failed to be controlled, or at least greatly reduced, by the remedies employed. I shall close by reporting two experiments the result of which, at first, seemed marvelous in comparison with

all we had thus far seen, and appeared far indeed from those of the second experiment.

I took the first box that fell into my hands. I divided it into two equal parts by means of a little wooden partition, which fitted quite well and prevented communication of one part with another. One side of this box was filled with new soil that I had taken from that thrown out of a town ditch. The other side was filled with this same kind of soil into which had been mixed some bunt spores at the rate of 1 to 5 (1 part bunt to 5 parts of soil). I sowed some clean wheat in the two parts of the box. The one produced only healthy heads; the other produced a large number of bunted heads.

I here present the second experiment. On the 19th of March, 1753, I transplanted in a pot, filled with soil that I was sure of, five wheat plants taken from a place, which, in my opinion, should produce only healthy plants. I watered these plants twice each day and until their condition was assured, with water into which I had thrown an abundance of smut and into which I infused a large amount of contaminated straw and bunt balls. To my great astonishment, none of these plants suffered from a watering that seemed must carry enough infection throughout to cause the plants to perish.

This singular fact shows that one must not make deductions too hastily. I considered my plants as lost in watering them thus with infested water, the condition of which was produced only by that same dust so fatal to the seed. But it is to be presumed that the wheat roots, once they have attained a certain stage of vigor, are safe against smut contagion, though it appears they are susceptible while they are still tender and delicate. In fact, it may be recalled that in plot 68 there were many bunted heads. This phenomenon doubtless came about only as the otherwise healthy roots came in contact, through growth, with the trail of smut that was only a little more than half an inch from the seed and there absorbed the poison, which the roots transmitted to the stem.

Continuation of the Second Project.

In proposing to the natural philosophers a subject so important as that of explaining the cause of the destruction of the wheat grain, etc. the Academy had two objects in view: Public welfare and the promotion of natural philosophy. It would, no doubt, be praiseworthy to satisfy both

perfectly. But, wherever, in my research, my efforts have not gone that far, we must admit that some helpful facts, althought a bit obscure, will, nevertheless, appear to have some value. The point under consideration possesses the particular merit of being of interest to all men as citizens and concerns only a small number among them as scholars. I shall, therefore, omit here any purely philosophical discussion. I shall, therefore, thenceforth avoid any and all purely scientific discussion. I shall risk no opinion that partakes of doctrine concerning the basic cause of the principal disease of wheat. It [the cause] may be of such nature, even, as to escape our recognition. In such respect it would have something in common with several causes of disease in the animal kingdom that cannot with certainty be determined, although the diseases, themselves, may be known to a certain degree and are often successfully treated.

It must suffice me, in considering the question from the practical side only, to be able to show in a positive manner and on the testimony of many experiments, (*1*) *that the common cause, the abounding source of bunted wheat plants resides in the dust of the bunt balls of diseased wheat; that the clean healthy seed, inoculated with this dust, receives through rapid contagion and a very intimate communication the poison peculiar to it; that it transmits the poison to the kernels of which it is the origin; that these kernels, once infested become converted into a black dust and become for others a cause of disease; that the culms, themselves, that bore the bunt heads contain something pestilential for the seed that lies near them and on which they germinate.*

In the second place, I am able to assert, *that the treatments I employed have protected the most heavily infested seed against the effects of the contagion, and that the success of these treatments has been so much the more positive even though the seed, on being treated, always retained the large quantity of smut with which it had been inoculated.*

Even though one judge my observations favorably and adopt all the facts that I have collected, one may still find oneself baffled by certain difficulties that it is well to disclose here. I shall have occasion, in seeking to resolve them, or at least in making them look like ordinary difficulties, to insist on certain points that have only been touched in passing and that, nevertheless, merit some discussion.

First Difficulty. Why, one may ask, in a field—sometimes, even, in a plot—where only clean, selected and sometimes treated seed had been sown—are there nearly always some bunted heads to be seen?

I shall first reply that no matter what attention one may give to the selection of the seed it is all but impossible not to find some kernel either wholly bunted or only partly so or some sound kernel that had made contact with a diseased one. In a sheaf that one has, oneself, prepared from the finest heads, there may be one containing two or three bunt balls. The dust that ruins these kernels, suffices to contaminate many others when the sheaf is threshed. It may be recalled in speaking of the clean selected wheat used in my 1752 sowings, that I noticed, in spite of my precautions, some bunt balls slip in. Could not their poison be as virulent for the healthy kernels nearest them?

In the second place, I shall say that it is not clearly determined that in our climate bunt originates by itself and independently of any contagious agent. A certain fact, determined by my own observation, is that one of the most skillful farmers of the neighborhood of . . . who devotes scrupulous attention to the choice of his seed never has any diseased wheat, although the different parts of the ground he has improved are situated some considerable distance apart and it would thereby seem should afford some opportunity for variation in the nature of the crop. A fact as certain as the first is that the farmer whose bunt-infected field was alongside mine in 1752 has had his wheat constantly damaged in several consecutive years through the practice (1) of sowing only the same seed—seed that, the first time grown by this farmer, was damaged by smut; (2) of using no other manures than those with which his straw was incorporated; and (3) by thus perpetuating bunt in several pieces of ground he had under cultivation.

Does not the condition of the wheat of these two farmers, so different for the sole reason that the one sowed only selected seed and the other for a long time had used only contaminated seed, lead you to suspect that smut is not of spontaneous origin, that it is an exotic disease in our clime, and does not occur here except through contagion?

In that respect it is similar to a formidable and unfortunately too common disease, which, from the time it ap-

peared in Europe, passed for an epidemic disease caused by bad atmospheric conditions; whereas today it is regarded as very certain that it does not develop at all unless the virulent principle is provided by a host already infected with the same disease and that, once this principle is eliminated, the disease is never produced by any other circumstance.[2]

In reading the work of the scholarly physician whom I cite here, one will note a sort of analogy between the disease of which he speaks and that of bunted wheat. For example, with reference to the former, has it not been observed that an ordinary disease principle is not invariably peculiar to such and such host; or that, if this principle, without exception, produces its effect, it is more widespread and marked on certain hosts than in others? Is it a question of the latter disease? One observed that not all the wheat grains that are uniformly blackened with smut are affected by the virus and that among the number of those that are affected, some produce completely bunted plants and others produce plants in which the disease extends only to a part of the heads or to some kernels within a single head. Whether one finds the virus innocuous or only partly effective or, again, wholly so, undoubtedly always depends relatively on the peculiar nature of the inoculated kernels and on the special disposition of the germ. Here we have a condition not unlike that peculiar to human beings exposed equally to attack by pests and without protection against them. Whether this or that terrible plague produce the effect of which it is capable or only a mild attack or none of any noticeable degree, will depend relatively on the condition of the body at the time or on its particular temperament. M. Austre.

Second Difficulty. If the straws drawn from bundles con-

[2] Ignorabatur luem Veneream, cum in Europâ primum apparuit; contagione traduci usu Veneris, aegrotis scilicet modum quo infecti erant cautè dissimulantibus, ut flagitia celarent; vel forsan ne suspicantibus quidem posse tam gravem morbum sola Venere contrahi, ut pote quae contagionis ratio planè insolens videretur. Indè adeò factum est ut Medici, qui illo tempore florebant summâ consensione crediderint ut jam supra dictum fuit, novum hunc morbum, more caeterorum pestilentium, Epidemicum, seu Popularem esse et induci proinde à causâ extremâ et communi qualem alii esse putabant noxium Astrorum influxum, vel Planetarum aspectum; alii verò insalubrem aëris statum, quem pluviae vel inundationes attulissent. . . . Sed pervicit tandem nota rec veritas ac jamdudum certâ, constanti, indubitatâ experientiâ innotuit, et unanimi Medicorum testimonio compertum est luem Veneream non concipi, nec errore dioetae, nec aëris vitio nec ullo rerum non naturalium abusu, nec spontanea humorum labe; *sed unicâ tantum ratione propagari si ab aegroto in sanum communicatione tradatur.* De morbis Venereis. Tom. I, pag. 119 & 120.

taining much smut are dangerous to seed sown in a dry condition, why do they occasion so little damage when buried or mixed with manure?

Assuming that the supposedly contaminated straw were impregnated with the kind of virus contained in the bunt dust, one should not be at all surprised by the condition of putrefaction to which they were found reduced in the manure. They lost the major part of it and, therefore, became less dangerous to the seed. In fact, we have seen that, following the use of a sort of composite manure, where rotting of the contaminated straw was incomplete, the kernels produced a large number of bunted heads.

But, if there be reason to hesitate in regarding the straw as susceptible to any virus, I shall still have a plausible explanation to give for the proposed difficulty.

In spite of the precaution I exercised in preventing any smutted heads from being incorporated with the straw I was not, however, able to prevent the straw from becoming contaminated with bunt dust when it was threshed and at which time the bunt balls were removed from the heads, and only the straw was saved. It is very evident that this fine dust was attached to the straw, for, when the straw was chopped to pieces, there arose from it a dust that entered the workman's throat and caused him to cough frequently.

That granted, I shall state that if the dry straw that was spread in the furrows was really contaminated with a little of the dust from the bunt balls, it is natural and in accord with numberless experiments that the soundest seed will suffer from proximity to such contaminated straw. But it is no less natural to assume that if the straw under suspicion possessed, in itself, nothing harmful and could harm the seed only through an extraneous dust with which it was contaminated, the dung mixed with this straw was more or less capable of absorbing this dust and of arresting its action.

In thus recognizing no bad intrinsic quality in the culms that had borne the bunted heads, one might very easily explain why, in that part of the tract infected with smut in 1752, the stubble I left there and on which I sowed some wheat produced no baneful effect. After all, leaving out of our consideration of the subject the possible fact that one would have nothing to fear from the stubble remaining in a

field where a large amount of smut had been harvested, it is quite inconsequential to a farmer whether his straw be dangerous in itself or whether it be only the cause of the dust with which it was contaminated. It is impossible for him to dodge the fact that, when he threshes the sheaves, the contaminating dust spreads to the healthy straw as well as the diseased. The only precaution this farmer is prepared to observe is that of not using manures with which suspected straw has been mixed or to use only a manure in which such straw had been completely rotted.

Third Difficulty. It has been suggested, and it is apparent, that the virus contained in the dust with which the seed was inoculated penetrated the seed, attacked the germ, infected the roots, became more or less active with the growth of the plant and became manifest in all its vigor, at first in its effect on the stamens and, finally, on the developing kernel. How can a wheat plant, within which this poison resides 7 or 8 months, carry on with only certain outward symptoms manifested by the culms and leaves (and, yet, a darnel plant, as we have remarked, shows none whatever in either the one or the other), while the kernels produced by the plant are found ruined before they are fully formed?

I shall not make a direct answer to this question; I shall limit myself to an attempt to establish the existence of a virus in the plant, although its presence there may be scarcely evident and all the disorder seems concentrated within the kernel itself, by calling attention to the fact that this, which appears to be peculiar to wheat plants attacked by bunt, is quite common in several of the diseases of animals. I shall cite only one of them. The parts of the human body have between them too many points of union. The solids and liquids of the body react with so much consonance that, in a man, attacked by gout, the entire mechanism does not suffer from the vicious causal principle. It is, however, in the joints where this malady has its location; rarely does it manifest itself elsewhere.

Fourth Difficulty. Why do not the remedies, with their very salutary effects on the seed *en masse*, extend universally to each and every grain rather than leave some vestiges of the disease?

One will be less surprised that the efficacy of the preparations (treatments) does not extend completely to all the seed when it will have been noted that I sought to bring into contact in the treated grain both the cause of the disease, such as has never been seen, and the remedy designed to prevent it. Surely, nobody would presume to bury his seed in bunt dust nor apply a remedy to seed thus inoculated. This test was, however, useful to me. I performed it in order that I might the more truly judge of the efficacy of the remedy. A farmer might never have occasion to employ it except on lightly inoculated seed and even then he might in the very beginning remove the little smut that adhered to the brush of the kernel by the simple act of washing the seed in ordinary water. Is it not fair to presume that the treatments will produce a more universal effect in the latter instance than they would in the special experiment I performed?

I should again note that in the best selected seed some kernels are sometimes found that, though apparently sound, are inwardly partly sound and partly bunted. However, they are quite capable of germination and subsequent production. The grain produced by plants grown from such seed suffers without fail from the defect of its origin.

The remedies I have employed have had their effect only insofar as the cause of the disease was present on the surface of the seed. Are these remedies sufficiently potent in those instances, still very few and concerning which one ought to be a bit concerned, where the cause must be met even within the interior of the seed? This is something I have not yet demonstrated. Moreover, in the large number of remedies I have employed in trying to prevent smut, there will be found, unless I am much mistaken, some one remedy of specific value against this disease to the extent that it nearly wipes it out.

It is true that three of these remedies, although apparently simple, and in the order of those that have given me such good success, have been so violent as to kill a large number of the diseased kernels to which I have applied them. But it is, nevertheless, well to have at hand some remedies capable of penetrating beyond the pericarp. Once their strength becomes known, it will be easy so to handle them as to render them salutary to the seed and to dilute

them to the point where the germ will not be affected and where the internal cause of the disease will be met.

If success authorizes me to recommend the remedies that I have latterly employed, this is a second benefit that will result from them and that it was necessary for me to keep in mind while carrying on my study of preventive measures. Most of these remedies will cost nothing; others will cost little; all will be easy to apply. I have been well aware of the fact that the farmer who is sometimes poor at figures and who does not willingly depart from his practices might not welcome even a slight expense, or he might be embarrassed by a preventive measure requiring too much work. I have, therefore, tried to follow his sense of economy, to approximate his practices, and to be useful to him without inconveniencing him.

First Project

PROJECT OF THE PLOT USED

Width—Twenty-four feet.

First Division Pigeon Manure						Second Division Sheep Manure						Third Fecal		
Oct. 16	Oct. 22	Oct. 27	Nov. 3	Nov. 10	Nov. 22	Oct. 16	Oct. 22	Oct. 27	Nov. 3	Nov. 10	Nov. 22	Oct. 16	Oct. 22	Oct. 27
Wheat blackened with bunt 1	Naturally contaminated 5	Artificially inoculated 9	Naturally contaminated 13	Artificially inoculated 17	Naturally contaminated and artificially inoculated 21	Artificially inoculated 25	Naturally contaminated 29	Artificially inoculated 33	Naturally contaminated 37	Artificially inoculated 41	Naturally contaminated and artificially inoculated 45	Artificially inoculated 49	Naturally contaminated 53	Artificially inoculated 57
Salted and limed 2	Salted etc. 6	Salted 10	Salted 14	Salted 18	Salted 22	Salted 26	Salted 30	Salted 34	Salted 38	Salted 42	Salted 46	Salted 50	Salted 54	Salted 58
Clean 3	Clean 7	Clean 11	Clean 15	Clean 19	Clean 13	Clean 27	Clean 31	Clean 35	Clean 39	Clean 43	Clean 47	Clean 51	Clean 55	Clean 59
Treated with Saltpeter and lime 4	Limed 8	Saltpetered etc. 12	Limed 16	Saltpetered 20	Limed 24	Saltpetered 28	Limed 32	Saltpetered 36	Limed 40	Saltpetered 44	Limed 48	Saltpetered 52	Limed 56	Saltpetered 60

One hundred and eight feet || One hundred and eight feet || One hundred

TOTAL LENGTH OF PLOT

IN THE EXPERIMENT OF 1751-1752

Division Matter			Fourth Division Horse and Mule Manure						Fifth Division Natural Soil					
Nov. 3	Nov. 10	Nov. 22	Oct. 16	Oct. 22	Oct. 27	Nov. 3	Nov. 10	Nov. 22	Oct. 16	Oct. 22	Oct. 27	Nov. 3	Nov. 10	Nov. 22
Naturally contaminated 61	Artificially inoculated 65	Contaminated and inoculated 69	Artificially inoculated 73	Naturally contaminated 77	Artificially inoculated 81	Naturally contaminated 85	Artificially inoculated 89	Contaminated and inoculated 93	Artificially inoculated 97	Naturally contaminated 101	Artificially inoculated 105	Naturally contaminated 109	Artificially inoculated 113	Naturally contaminated 117
Salted 62	Salted 66	Salted 70	Salted 74	Salted 78	Salted 82	Salted 86	Salted 90	Salted 94	Salted 98	Salted 102	Salted 106	Salted 110	Salted 114	Salted 118
Clean 63	Clean 67	Clean 71	Clean 75	Clean 79	Clean 83	Clean 87	Clean 91	Clean 95	Clean 99	Clean 103	Clean 107	Clean 111	Clean 115	Clean 119
Limed 64	Saltpetered 68	Limed 72	Saltpetered 76	Limed 80	Saltpetered 84	Limed 88	Saltpetered 92	Limed 96	Saltpetered 100	Limed 104	Saltpetered 108	Limed 112	Saltpetered 116	Limed 120
and eight feet			One hundred and eight feet						One hundred and eight feet					

FIVE HUNDRED AND FORTY FEET

SECOND PLAT — PLAT OF GROUND USED IN

Clean wheat without manure 8 bunted heads, four [loose]-smutted. 1	Clean wheat without manure. 5 bunted heads, 4 [loose]-smutted. 6	Clean wheat without manure. 11 bunted and 2 [loose]-smutted heads. 11	Clean wheat without manure. 6 bunted heads, 2 [loose]-smutted. 16	Clean wheat without manure. 1 bunted head, 4 [loose]-smutted. 21
Clean wheat, horse manure made of clean straw. 14 bunted heads, 2 [loose]-smutted. 2	Horse manure made of clean straw. 6 bunted heads, 1 [loose]-smutted. 7	Horse manure and clean straw. 2 bunted and 3 [loose]-smutted heads. 12	Manure comprising sheep manure and healthy straw. 19 bunted heads, 3 [loose]-smutted. 17	Manure consisting of cow dung and clean straw. 2 bunted heads, 3 [loose]-smutted. 22
Clean wheat, horse manure made of clean straw. 4 bunted heads and 2 [loose]-smutted heads. 3	Horse manure made of straw of doubtful freedom from bunt. Fewer bunted heads than in the plots thus fertilized, 4 [loose]-smutted heads. 8	Horse manure and healthy straw. 1 bunted head and 3 [loose]-smutted heads. 13	Manure comprising sheep manure and healthy straw. 94 bunted heads, 4 [loose]-smutted. 18	Manure comprising cow dung and clean straw. 32 bunted heads, 7 [loose]-smutted. 23
Clean wheat, horse manure made of straw of doubtful freedom from bunt. 48 bunted heads, 2]loose]-smutted. 4	Manure composed of sheep droppings and clean straw. 1 bunted head, 2 [loose]-smutted heads. 9	Horse manure and suspected straw. 11 bunted and 6 [loose]-smutted heads. 14	Manure comprising sheep manure and suspected straw. 825 bunted heads, 3 [loose]-smutted. 19	Manure comprising cow dung and suspected straw. 29 bunted and 4 [loose]-smutted heads. 24
Clean wheat, horse manure made of straw of doubtful freedom from bunt. 49 bunted heads, 3 [loose]-smut. 5	Manure composed of sheep droppings and straw of doubtful freedom from bunt. 10 bunted and 1 [loose]-smutted heads. 10	Horse manure made of suspected straw. 22 bunted heads. 15	Manure comprising sheep manure and suspected straw. 879 bunted heads, 2 [loose]-smutted. 20	Manure comprising cow dung and suspected straw. 30 bunted and 3 [loose]-smutted heads. 25
Healthy darnel on clean soil. 141 healthy heads, 1 ergot, no bunt. A	Healthy darnel contaminated with the dust of bunted darnel and treated with saltpeter and lime. 240 healthy heads, 1 ergot, 0 [loose] smut. B	Healthy darnel in soil inoculated with bunt of wheat. 1 ergot, 0 bunt. C	Darnel inoculated with bunt of wheat. 230 sound heads, 0 bunt. D	Sound darnel, contaminated with darnel bunt. 224 sound heads 96 bunted heads 320 heads E

WIDTH 48 FEET

First part of the plat worked with the plow; 116 feet long by 12 feet wide.
Clean wheat, well washed and dried in the sun, although the heads from which this grain came were selected from very sound sheaves.

RESULT

Very good: Bunted heads extremely rare.

THE EXPERIMENTS OF 1752-1753

Clean wheat without manure. Sound straw cut-in-pieces. 2 bunted heads and 1 [loose]-smutted head. 26	Clean wheat without manure. 5 bunted and 3 [loose]-smutted heads. 31	Clean wheat without manure. 2 bunted and 2 [loose]-smutted heads. 36	Clean wheat without manure. 3 bunted and 3 [loose]-smutted heads. 41	Clean wheat; without manure. 3 bunted heads. 46
Suspected straw. 27	Clean wheat, dampened with mucilage and inoculated with bunt. At least ⅓ of the heads, bunted. Poor (thin) stand. 32	Clean wheat, inoculated with bunt, dampened, treated with salt-lime solution without losing any of the bunt. 10 bunted and 3 [loose]-smutted heads. Quite good. 37	Clean wheat, inoculated with dry bunt and buried in the dust. 566 healthy plants, 491 diseased plants 1057 2026 sound heads 1687 bunted heads 3713 heads 42	Clean wheat, contaminated with darnel dust, inoculated with wheat bunt and buried in the dust. Affected like 42, 43, 44, & 45 but to a less degree. More delay in the evidence of bunt. 47
Suspected straw. 28	Clean wheat, dampened with mucilage and inoculated with bunt. ½ the heads bunted, five heads of loose smut. 33	Clean wheat, inoculated with bunt, moistened, treated with salt-lime solution in this condition without loss of bunt. 8 bunted heads, 3 or 4 [loose]-smutted. Excellent. 38	Clean wheat, inoculated with dry bunt and covered at a depth of one to two inches. ⅓ or less of bunted heads, 6 [loose]-smutted. Poorer than the preceding. 43	Clean wheat, contaminated with darnel dust, inoculated with bunt, and buried at a depth of 1 or 2 inches. 778 healthy plants 365 diseased plants 1143 plants 2194 sound heads 898 bunted heads 3092 heads 48
Sound, threshed heads and sound kernels. 29	Clean wheat dampened with fish glue and inoculated with bunt. ½ or nearly ½ the heads bunted. 34	Clean wheat, inoculated with bunt, dampened, treated with saltpeter and lime without loss of bunt. 3 bunted heads. Some [loose] smut. Excellent. 39	Clean wheat, inoculated with dry bunt and buried at a depth of 3 or four inches. ½ of all heads bunted, 5 [loose]-smutted. Not so good as the 43rd plot. 44	Clean wheat, contaminated with darnel dust, bunted, and covered at a depth of 3 or 4 inches. ⅓ of the heads bunted. 49
Threshed bunted heads and questionable kernels. 30	Clean wheat dampened with fish glue and inoculated with bunt. ½ of the heads bunted. 35	Clean wheat, inoculated with bunt, dampened and then treated with saltpeter and lime without loss of bunt. 84 bunted heads, 3 or 4 [loose]-smutted heads. Excellent. 40	Clean wheat inoculated with dry bunt and buried at a depth of 5 or 6 inches. 70 sound plants 211 diseased plants 281 plants 331 sound heads 938 bunted heads 1269 heads 45	Clean wheat contaminated with powder of darnel, inoculated with bunt and covered at a depth of 5 or 6 inches. 318 sound plants 344 diseased plants 662 plants 903 sound heads 998 bunted heads 1901 heads 50
Sound darnel, contaminated with darnel bunt and washed. 217 sound heads 129 bunted heads 346 heads F	Darnel collected in a field where there was much darnel attacked by bunt. 463 sound heads 1 bunted head 464 heads G	Clean rye in non-contaminated soil. 175 healthy heads. H	Clean rye in soil inoculated with wheat bunt. 31 sound heads, 1 ergot. I	Clean rye, inoculated with wheat bunt. 144 sound heads, 4 heads affected with ergot. K

First division containing 4 rows of clean barley.
No disease.
Six feet.
feet.

Second part of plat, worked with the plow; same dimensions as the first. Same seed as in the first part, with this difference, namely, that it had been heavily inoculated with bunt on Oct. 4, left, thus contaminated, enclosed in a box, until the 24th of Oct., when it was well washed, exposed to the sun to dry and sown the following day. One could see by the aid of the low power of a microscope, however, some particles of bunt in the brush of the washed kernels which the washing did not remove.

RESULT

Quite good: A large number of bunted heads.

Continuation of the Second Plat

Clean wheat 8 bunted heads, 2 loose-smutted. 51	4 bunted heads. 56	6 bunted heads. 61	2 bunted heads. 66	3 bunted heads. 71
Clean wheat, on horse manure made of suspected straw. 40 bunted heads, 6 loose-smutted. 52	Clean wheat, inoculated with bunt, No. 4. 2/5 of the heads bunted. One loose-smutted. 57	Beardless wheat from a field infected with bunt. A few bunted heads and some loose smut, enough, indeed, to stigmatize this kind of wheat. 62	Clean wheat, in furrows the bottom of which had been dusted with bunt. Many bunted heads. 67	Clean wheat, salted and limed. Some bunted heads, otherwise excellent. 72
Clean wheat, on horse manure made of suspected straw. Almost like the 52nd. 53	Clean wheat, inoculated with loose smut. Quite a large number of bunted heads. Three loose-smut heads. Quite good. 58	Beardless wheat from a bunt-infected field. Same observation as in plot 62. 63	Clean wheat, in rows where a mere line of bunt had been run approximately ½ inch from the seed. Many bunted heads. 68	Clean wheat, saltpetered and limed. Some bunted heads. Otherwise very good. 73
Clean wheat, no manure. Four or five bunted heads. 54	Clean wheat inoculated with barley smut. Several bunted heads. Two loose-smut heads. Quite good. 59	Bearded wheat from a field infected with bunt. Same observation as in the two preceding plots. 64	Beardless wheat, dusted with bunt. 1641 sound heads 1476 bunted heads 3117 heads 69	Clean mouse wheat. No bunted heads. 74
Wheat from partly smutted, partly sound heads, picked out by hand and washed. Very few bunted heads. One loose-smutted. 55	Clean wheat inoculated with oat smut. Few bunted heads. Two loose-smutted. Excellent. 60	Bearded wheat, from a field infected with bunt. Same observation, etc. 65	Bearded wheat inoculated with bunt. 1624 sound heads 1705 bunted heads 3329 heads 70	Mouse wheat, inoculated with bunt. A few bunted heads. 75
Rye dusted with the powder of ground ergots 286 healthy, 8 ergoted heads. L	Clean common barley in noncontaminated soil. No disease M	Clean common barley in furrows the bottoms of which had been dusted with bunt of wheat. No disease. N	Clean common barley dusted with bunt. No disease. O	Clean common barley, moistened, and then blackened with bunt. No disease. P
			Second division containing 4 rows of 4-row barley inoculated with bunt of wheat. No disease 6 feet	

360 FEET

Continuation of the Same Plat

Contaminated Carthusian wheat, washed. Several bunted heads. 76	Clean mouse wheat. Several aborted heads. 81	Clean mouse wheat. A single blighted head. 86	Clean mouse wheat. Some aborted heads. 91	Clean mouse wheat. Several heads in an aborted condition. 96
Contaminated Carthusian wheat, saltpetered and limed. Very few bunted heads. 77	Spring wheat, picked by hand, unwashed. Some bunted heads. Some loose-smut heads. Mediocre. 82	Clean common barley. Six smut heads. 87	Clean spring wheat. 1484 sound plants 8 diseased plants 1492 plants 3124 sound heads 15 diseased heads 3139 heads 92	Clean oats. 4 or 5 smutted panicles. 97
Contaminated Carthusian wheat, salted and limed. Very few bunted heads. 78	Same spring wheat, dry, inoculated with bunt. ½ the heads bunted. Some loose-smut. Mediocre. 83	Clean common barley, dry, inoculated with spores of smut of barley. 39 smutted heads. 88	Clean spring wheat with suspected wheat straw. 2025 sound plants 676 diseased plants 2701 plants 3215 healthy heads 1165 bunted heads 4380 heads 93	Oats, dry, dusted with spores of oat smut. Like the preceding. 98
Contaminated Carthusian wheat, nontreated. A large number of bunted heads. 79	Same spring wheat, moistened with mucilage solution and dusted with bunt. 3/5 of heads bunted. Some loose smut. Mediocre. 84	Clean common barley, dry, dusted with bunt of wheat. Six loose-smutted. 89	Spring wheat, dry, dusted with bunt. ½ the heads bunted. Several loose-smutted. Weak and short in spots. 94	Oats, dry, dusted with bunt. Like the preceding. 99
Some contaminated Carthusian wheat, nontreated. A large number of bunted heads. 80	Miracle wheat, without treatment. No disease, excellent. 85	Clean common barley, dry, inoculated with barley smut and treated with saltpeter and lime in this condition. 35 heads of smut. This and the three preceding plots were very fine. 90	Spring wheat, blackened with bunt, dry, treated with saltpeter and lime in this condition. Few bunted heads. Some loose-smutted, Seven or 8 inches taller than the preceding plot. 95	Oats, dry, inoculated with oat smut spores and treated with saltpeter and lime. Like the preceding. These last four plots were uniformly excellent. 100
Clean common barley, dusted with common-barley smut. No disease. Q	Healthy common barley, moistened and dusted with spores of common-barley smut. No disease. R	Healthy common barley, blackened with bunt, treated with saltpeter and lime in this condition. No disease. S	Mouse wheat moistened with mucilage solution and blackened with bunt. Several heads smutted but by no means so many as in a plot of winter wheat dusted with bunt. T	Clean mouse wheat. 1 or 2 bunted heads. Some crinkled plants. V

Third part of the plat worked with the plow, having the same dimensions as the two preceding parts.

The same seed as in the Second part, with this difference that, after having washed it superficially, it was dusted while still moist, with new bunt, in order that it might the better adhere to the seed.

RESULT

A large number of blighted plants. Many weak and short culms. A prodigious quantity of bunt-infected plants. In seizing a handful of heads, one could find scarcely three or four sound ones.

OVER ALL

Third Plat

	Row	
	Row 1	Clean wheat
	" 2	" "
	" 3	" "
	" 4	" "
A	" 5	Clean wheat, dusted with bunt
	" 6	" " " " "
	" 7	" " " " "
	" 8	" " " " "

	Row	
	Row 1	Clean wheat
	" 2	" "
	" 3	Clean wheat, moistened with mucilage solution, and dusted with bunt
	" 4	" " " " " " " " " "
B	" 5	Clean wheat, from heads that were partly bunted and partly sound
	" 6	Contaminated Carthusian wheat, washed
	" 7	" " " nonwashed
	" 8	" " " "

Results of the two plats of the First Square

	Healthy wheat plants	Sound heads produced by these plants	Diseased wheat plants	Bunted heads produced by these plants
Row 1	91	478	3	16
" 2	113	427	1	4
" 3	131	482	1	3
" 4	123	511	1	9
A	458	1898	6	32
" 5	29	142	67	369
" 6	27	115	93	443
" 7	32	124	94	399
" 8	24	96	79	369
	112	477	333	1580
" 1	88	366	9	29
" 2	107	545	4	23
	195	911	13	52
" 3	29	155	87	493
" 4	33	156	81	479
B	62	311	168	972
" 5	98	507	8	45
" 6	101	409	7	36
" 7	72	249	32	127
" 8	67	271	49	203
	139	520	81	330

Fourth Plat. Plan of the Second Square

C	Row 1	Clean wheat
	" 2	" "
	" 3	Clean wheat, dusted with bunt
	" 4	" " " " "
D	" 1	Clean wheat, dusted and washed
	" 2	" " " " "
	" 3	Wheat, bunt-dusted after having been moistened
	" 4	" " " " " "
E	" 1	Clean wheat, on horse manure into which clean straw had entered
	" 2	" " " " " " " " " " "
	" 3	Clean wheat on horse manure into which suspected straw had entered
	" 4	" " " " " " " " " " "
F	" 1	Clean darnel grass seed
	" 2	" " " "
	" 3	Darnel seed, inoculated with spores of darnel smut
	" 4	" " " " " " " "

G

1	2	3	4	5	6	*	1	2	3	4	5	6
Clean wheat, on healthy straw, dry	ditto	ditto	ditto	ditto	ditto	Darnel inoculated with spores of darnel smut	Clean wheat on dry suspected straw	ditto	ditto	ditto	ditto	ditto

Result from the five plots of the Second Square

Plot	Row	Sound heads	Bunted heads
C	Row 1	461	25
	" 2	431	52
		892	77
	" 3	43	429
	" 4	78	602
		121	1031
D	" 1	233	562
	" 2	235	371
		468	933
	" 3	35	457
	" 4	48	702
		83	1159
E	" 1	542	153
	" 2	404	91
		946	244
	" 3	361	151
	" 4	411	284
		772	435
F	" 1	343	2
	" 2	484	11
		827	13
	" 3	22	455
	" 4	39	444
		61	899

G

	Sound heads	Bunted heads
	97	2
	81	0
	131	2
	136	0
	165	5
	162	2
	772	11
*	11	110
	143	14
	109	22
	133	27
	127	14
	145	27
	163	18
	820	122

CONTINUATION

OF THE

EXPERIMENTS and REFLECTIONS

RELATIVE *to the Dissertation on the cause of the corruption and smutting of the kernels of wheat in the head, and on the means of preventing these unfortunate results,*

WHICH, in the judgment of the Royal Academy of Literature, Science and Art of Bordeaux, won the Prize.

By

M. TILLET, *Director of the Mint of Troyes*

IN PARIS

Chez BRIASSON, Libraire, rue Saint Jacques, à la Science

MDCCLV

With the Approval and Authorization of the King.

CONTINUATION
OF
EXPERIMENTS
AND
REFLECTIONS
ON

the Cause of the Corruption and Smutting of the Kernels of Wheat in the Head and on the Means of Preventing these Untoward Circumstances

IN the experiments that I performed in 1752 to 1753, and of which the second part of my dissertation presents the details, my principal object was to inoculate the purest and most select seed by blackening it with the spores from bunt balls obtained from smutted wheat, and thus produce a large quantity of smutted heads by sowing this inoculated seed, and thereby leave no doubt as to the hidden malignity of the dust with which I had blackened it.

In the experiments of 1753-54, which I am here about to describe, I had as my principal object, on the contrary, to employ only the most contaminated seed, that most laden with smut on leaving the fan and to subject it to different preparations in order to protect it against contagion, and to prevent any transmission of the disease to the heads that might come from such seed. In a word: I sought first to establish the principle of the disease; then, finally, my thought was directed toward preventive materials.

Among the remedies I shall mention, several have been wholly successful. The reader, moreover, is already aware of the fact that they possess advantages in which I was particularly interested—advantages that will leave the negligent farmer without any excuse. These remedies require little care; they cost nothing, or are held at a most moderate price.

When one knows the characteristics of the farmers, their sometimes poorly comprehended economies, their predilection for their own practices, and their hesitancy to re-

ceive anything not conceived by someone in their midst, one feels that it would be necessary, if he assure himself of their tractableness, to put them to no expense in acquiring something of value to them, to avoid requiring their departure from a certain round of operations to which they are committed and trick them, so to speak, in abandoning them, to look into scarcely any difference between the precautions one counsels them to observe and those to which they always have been constrained to practice.

The order I have followed in reporting my first experiments, being the simplest and most natural, I shall follow in describing these. I shall begin by giving a succinct account of the preparations required for the composition of the several fertilizers; I shall give the distribution of the plots where my tests were made; I shall tell of the different kinds of seed and even of the differently prepared lots of seed sown; I shall place each kind in its original order.

The observations that I had time to make while the wheat was growing, being the same as those presented in the body of my dissertation, I shall pass at once to the condition of the wheat already in head, to the outcome of my plots, to that obtained from a plot worked in accordance with the ordinary method, and to the deductions that I shall draw from it.

I shall refer next to some homemade experiments.

They were conducted in the same garden already under consideration: the account of them will be complete. From now on I shall not adhere to a certain precision in speaking of the operations carried on in the open country. It will be best if I render a more exact account of the results of experiments performed in the city and not limit myself to presenting simple estimates. Moreover, the experiments, on the whole, are as well adapted as I could desire to the small-scale tests I have observed daily and that it is easy for me to describe carefully.

The course I am following in this new supplement shows clearly that I do not write gracefully. My plan, in fact, is to present only the series of my operations, to make clear my thoughts, wherever they may lead me, and to develop my work well by placing the facts before the eyes of the reader precisely as they have passed under mine. First, he will find himself at the time of seeding. He will be a witness to my

preparations. He will follow me in my several seedings, and will see nearly all without yet understanding the outcome of them.

Then he will find himself arrived at the time when the ground is covered with heads of wheat and when their condition is fully established; he will recognize each plot, each parcel of ground, worked with the plow; he will recall the kind of seed sown in each and the sort of preparation accorded each lot of seed; he will examine the plants from the standpoint of their health or from that of the damage sustained by them and will judge without confusion concerning the efficacy of the remedies applied.

I admit that in the matter of observations this procedure is a bit slow and that it requires some steadfastness, the reader may be obliged sometimes to retrace his steps. But I beg of him to note that I touch upon a very important matter of natural philosophy and concerning which, until now, there has prevailed much obscurity; that I must, as a consequence, occupy myself less with the construction of the discourse than with the manner of presenting the subject matter with exactness; and that it would be difficult for me, to attain this end, not to make some repetitions under the necessity of having to relate a large number of operations and a multitude of facts that require a presentation of the minutest details.

We have seen from the results of the experiments of 1752-53 that in the third part of the little parcel of ground worked with the plow there was a prodigious quantity of bunted heads. It was the most severely infected sheaves of this particular canton that supplied me with the straw that was later to be incorporated in the manure and designated *suspected* and the seed to which I applied different preparations to protect it against every unfortunate circumstance.

On the 27th of August, 1753, I mixed with two lots of horse droppings equal amounts of chopped healthy straw and suspected straw, one lot containing chopped healthy straw only, the other lot only contaminated or suspected straw. I put each of these mixtures into a special vat, throwing in enough water to wet the straw down well; and I left them thus until the 15th of October, on which date I used my fertilizers.

On the 30th and 31st of August and the 6th and 12th of

September I mixed some clean straw, as well as some suspected of being contaminated, with equal parts of rich sheep manure, dry and reduced to dust. These mixtures I combined with cow dung. They were put in equal amounts into special vats and were watered, as were the other two. They were allowed to remain in this condition until I used them.

The lots of suspected straw incorporated with the different manures in 1753 did not contain all of the bunted heads, as did those which entered into the mixtures of 1752. However, they would have produced an obvious effect, for they came from an infested canton where sound heads were rare. Moreover, in a sheaf full of smut the few sound culms present, when threshed, would soon share the dangerous condition of those bearing smutted heads, for we infer, from all appearances, that the suspected straw, in itself, contains nothing contagious and is to be feared only because it is laden with the dust from the bunt balls.

It may be recalled that from the little field where my experiments were carried on in 1752 I reserved a part for those projected for the next year. This portion was 26 feet wide by 360 feet long. I left out two feet of the width and it will soon be seen how it was utilized.

In this reduced area I made 92 plots. They were arranged in 4 parallel series, 23 plots per series. Each plot was 15 feet long by 5 wide and a space of one foot was left between parallel series to serve as an alley lengthwise of the area. By making my plots three feet shorter than those of former years I was able to increase the number and I managed, moreover, to reserve a strip 15 feet wide across the entire width of the nursery, where I put in 8 rows of wheat and 8 of darnel.

These rows were sown during the course of the month of September; that is, from the first to the 29th. Following is the order in which I laid them out, the kind of seed sown, and the day on which it was sown. The wheat I used for this little parcel of ground was clean and carefully selected. The first row was sown on the first of the month; so also was the second row, after the seed had been blackened with the dust from the grains of bunted wheat.

Seed from the same lot, clean and smutted, sown on the 7th, in the order indicated elsewhere, comprised the 3rd and 4th rows.

On the 14th two additional rows were sown: I always observed the practice of sowing first a row of clean seed and then a row of smutted seed. The same operation obtained on the 22nd; the very choice wheat and the same blackened with smut were sown alternately.

The 9th and 10th rows were sown on the 24th. They contained equal quantities of clean darnel seed and seed of the same kind contaminated with the dust of bunted grains of wheat.

The wheat comprising the 11th, 12th, 13th, 14th, 15th, and 16th rows was sown on the 29th of September. This seed, sown sometimes clean, sometimes smutted, was sown in the same order as that already referred to, in relation to the first rows.

I did not continue these plots into the month of October, as it seemed but natural I should do: I would have returned at the regular time for sowing, since my object was to be forehanded. The first rows of this little nursery gave me opportunity to make a quite helpful observation, but different from the one I expected to make and that I needed. Rarely in the performance of experiments does one's labor answer to no purpose if one pursues it without distaste and always with his eyes open: one seeks a certain truth, it escapes him; one is not at all concerned about another, it impresses him.

The various fertilizers, the composition of which has been noted in the foregoing, were applied to the first 20 plots and, as in 1752, spread in the rows that had been opened to a depth of four or five inches.

The 1st and 2nd plots contained cow manure with which clean straw had been mixed.

In the 3rd and 4th I put the same kind of manure but with it I had mixed the suspected straw.

The manure consisting of horse droppings and clean straw was applied to the 5th and 6th plots.

To the 7th and 8th plots I applied manure consisting of horse droppings and suspected straw.

The rich sheep manure, with which I had mixed clean straw, was applied to plots 9 and 10.

The same sheep manure, mixed with supposedly smut-contaminated straw, was put in rows 11 and 12.

Plots 13 and 14 contained dry sheep manure mixed with clean straw.

Plots 15 and 16 were given the same sort of sheep manure mixed with suspected straw.

The 17th and 18th plots received an application of fertilizer consisting of sheep manure plus clean straw.

The 19th and 20th plots were fertilized with sheep manure mixed with supposedly contaminated straw.

My object having been again to see whether the supposedly contaminated straw, once incorporated with the dung of the different animals, exerted any harmful effect upon the seed proximate to it, it was thought best to employ in these first 20 plots selected seed so that it could not, in itself, be considered the active agent of any untoward circumstance.

I commonly limited myself to five rows of grain to each plot in order that the individual wheat plants might have every opportunity to grow and become vigorous. I put 6 of them in 12 successive plots in order to multiply the experiments and to afford room, when I desired it, for some varieties in one and the same plot to bring about certain differences between the 3 first rows and the 3 last ones.

The 21st plot contained rye. In the 3 first rows the seed was clean and nontreated. That of the 3 last was contaminated with the dust that had been produced by some crushed rye ergots.

I smutted some rye seed with the dust from some bunted wheat and sowed it in the 22nd plot.

The 3 first rows of plot 23 consisted of clean, nontreated 4-row barley. The 3 last rows were sown with similar clean seed smutted with the dust from bunted wheat grains. Likewise, all the rows of plot 24 were sown to similarly contaminated seed.

In the 3 first rows of plot 25 I sowed clean seed of darnel grass; in the remaining 3 rows I sowed seed of the same kind, inoculated with the dust of smutty darnel grains.

The 26th plot was a duplication of the 25th. Excepting the darnel seed sown in the 3 first rows of plot 32, the seed of the 6 remaining plots was always contaminated; that is to say, infested with the dust of the smut peculiar to the seed to which it was applied.

I sowed the first part of plot 27 to artificially inoculated

darnel seed after having moistened it, in this condition, with ordinary water containing nitre and dusted it with lime. Instead of nitre I employed common salt as a preservative of the darnel seed I sowed in the second part of the same plot.

The darnel, with which I sowed the three first rows of the 28th plot, I moistened with water that had leached through salsola (saltwort) ashes. That of the three last rows I dampened with leach from pearl ashes. [Chickweed]. Following is the simple method by which I obtained these detergent waters; it was the same as that that I employed in obtaining leaches from potash and the leached ashes of green wood.

I reduced to powder a pound of saltwort and a like amount of pearl ashes; I put each in two pints[1] of water, which I boiled for 30 minutes or thereabouts. I then filtered it all through gray paper. I drew from the soda leach about 3 *demi-setiers* of a very limpid solution; from the pearl ashes, on the contrary, I drew off a somewhat foul liquid. These solutions, as one may well judge, were more or less acrid and penetrating, depending on the amount of the specific salt each kind of ashes contained. After the evaporation of 3 *demi-setiers* of liquid which resulted from the leaching of each kind of ashes I found that, per pound, I had in precipitated salt:

	ounces	drams	grans i
of potash	8	0.5	0
of saltwort ash	4	5.5	18
of pearl ash	1	7.5	0
of green-wood ash	0	7.5	0
A pint of our domestic leach yielded also a precipitate of...	0	2.0	0

The 29th plot was sown to darnel seed that I had moistened as much with the green wood-ash leach as with an ordinary homemade leach made by bleachers.

The 30th plot contained, in the first 3 rows, some darnel seed moistened with human urine voided in the morning, accumulated to a certain amount, exposed to the sun for 3 months and arrived at a certain degree of putrefaction. In the last 3 rows I sowed some darnel seed that I had moistened with ordinary water, into which had been mixed a little of spirits of nitre, and that I had subsequently dried by sprinkling over it some finely sifted quick lime.

[1] The Troye pint is greater by a fifth than that of Paris.

In the 31st plot I sowed equal amounts of contaminated darnel seed, one lot washed with ordinary water, the other with pure water. Both lots were then moistened with the leach from saltwort ashes and afterwards dried with lime dust.

The darnel sown in the first part of the 32nd plot was previously blackened with dust from bunted wheat grains. That sown in the remaining portion of the plot had been dampened with a mixture of 1 part of spirits of nitre and 7 parts of water, and afterwards dried with green-wood ashes.

The experiments performed in the 4 succeeding plots were repetition of those I had conducted in 1752 in plots 26, 27, and 28. They seemed to me to merit some consideration.

After having made 5 furrows, 4 or 5 inches deep, in the 33rd plot, the chopped wheat straw was placed in them. The straw was sound and mixed with it were the threshed wheat heads. This chopped straw was mixed with a little soil so that the roots of the growing wheat plants could find nourishment there. Selected seed was sown on this straw after which it was covered with soil.

The seed was the same for the 4 plots containing the different kinds of straw.

The 34th and 35th plots contained supposedly contaminated straw with heads. Not all of these latter had produced bunt balls; yet, in these plots there was subsequently observed the greatest number of them.

For the 36th plot I used only contaminated straw from which the heads had been removed.

I sowed Miracle wheat, blackened with bunt dust, in the 37th and 38th plots.

During harvest I was taken into one of the best wheat sections in the neighborhood of Troyes, and there I, myself, collected from standing grain some bundles of very healthy heads that, moreover, had not been near any diseased heads. From my little harvest I obtained seed that was as pure as it was possible to get and I sowed it, without treatment, in plot 39.

I used some of this seed also for the 40th plot after having moistened it with potash leach and sprinkled it with powdered lime.

The question now before us, for many of the plots, concerns the use of seed from the infected sheaves of which

I have already spoken. One may easily imagine that the seed obtained from such bundles would be covered with the contagious dust of the bunt balls and that mixed with the seed might be some bunt balls that the flail had not crushed.

In the 41st plot I sowed a portion of this wheat just as it was on coming from the fan and still black enough to soil the hand of the sower.

The greater part of this same grain was washed in ordinary water, care being exercised to change the water several times until it came away from the vessel in its natural clearness.

I sowed this washed seed in plot 42 and divided the plot off very neatly. From this plot, up to and including the 64th, it will be necessary always to infer use of the same seed washed in ordinary water before subjecting it to any other preparation.

Plot 43 contained seed that had been washed in ordinary lye water.

Plot 44 was sown with seed over which I had poured some lime water that was almost boiling and that contained no other ingredient.

The seed of the 45th plot was soaked in ordinary lye water and then dusted with lime.

For the 46th plot the seed was dampened with lye that had filtered through green-wood ashes, and was then dusted with lime.

I used lye from pearl ashes, and powdered lime, for the preparation of the seed for plots 47 and 48.

Lye from saltwort ashes, and powdered lime, were employed in preparing the seed for plots 49 and 50.

Potash solution, combined always with powdered lime, was used in treating the seed for plots 51 and 52.

The seed consigned to plots 53 and 54 was moistened with a saturated common-salt solution and then dusted with lime.

For the 55th and 56 plots there was this difference in the treatment of the seed, namely, that instead of a solution of common salt, I used a saturated solution of saltpeter.

The seed sown in plot 57 I soaked in slightly warmed putrefied cow urine. I made use of this same urine, combined with powdered lime, in the treatment of the seed sown in plot 58.

Putrefied, slightly warmed human urine was used both alone and finally absorbed by powdered lime in treating the seed for plots 59 and 60.

I soaked the wheat for plot 61 in lukewarm, thick, liquid manure.

That sown in the 62nd plot was dampened in the same way and then dusted with lime.

The seed in plot 63 was given a cold bath in a solution of 1 part spirits of nitre and 9 parts of ordinary water.

The seed for the 64th plot was washed in a warm solution comprising a mixture containing about an eighth part more of the saltpeter. The seed was subsequently dusted with lime.

From the preceding year I had a remnant of some selected wheat and a little of this same wheat, still covered with bunt dust. Plot 65 received the clean wheat, and plot 66 the contaminated seed.

After having washed this same seed quite superficially, inoculated since 1752, I dampened it with a potash solution, dusted it with lime, and sowed it in the 67th plot.

Without washing, some of this same seed, retaining all the contagious dust with which it was blackened was moistened with a saltpeter solution, dusted with lime, and sown in the next plot.

The 69th plot contained clean wheat obtained from sound heads that I had carefully picked out and collected from the plants while harvesting.

The 70th was of the same wheat, contaminated with the dust of smutty darnel seed.

The 71st plot was sown to some of the same seed used in the preceding plot but dampened with potash solution and dusted with lime.

The 72nd contained some of this same lot of seed inoculated with the dust of darnel smut, but dipped afterwards in saltpeter solution that was subsequently absorbed by powdered lime.

In plot 73 I repeated the experiment of communicating the smut disease through the newly developed roots of selected seed by putting a line of the bunt dust in the drill row where the seed was sown, but seven or eight lines [7/12 or 2/3 of an inch] distant from the seed, and by observing the precautions of which I spoke in the second part of my dissertation (page 112).

The furrow bottoms of the 74th plot were contaminated with the same contagious dust with which, in the preceding plot, I had made only some mere tracings. In this plot I sowed clean seed, free from any suspicion of contamination, on this dust. The seed of the 3 first received no treatment; in the 2 last rows it had been dampened with saltpeter solution and dusted with lime.

A scholar has conceived the idea that the dust contents of bunted grains of wheat might consist of an innumerable multitude of eggs produced by insects, whence might emerge other tiny animals capable of perpetuating the disease of which their parents were supposed to have been the cause. It was necessary either to give this conjecture some support or disprove it absolutely. It was still of interest to observe whether the dust in question, if subjected to a lively heat would lose its contagiousness. I subjected it to different degrees of heat. I took a little silver vessel, nearly the shape of a goblet, and filled it with this dust. I put the vessel in a sand bath, which I slowly heated. I plunged a mercury thermometer into the dust and removed the little vessel when it had acquired a heat of 20 degrees.

The vessel was refilled with dust, placed in the sand bath, and withdrawn when the thermometer, plunged into the dust as in the first instance, showed a temperature of 40 degrees.

Some new dust, being put into the little dish, was heated a long time, notwithstanding the fact that the surrounding sand attained an ultimate temperature of 60 degrees.

It was much more difficult for me to raise the heat to the point of boiling water. After having increased the fire under the sand and waited almost a half hour to attain this end I was obliged to abandon the sand bath as a means of heating the bunt dust. A matter worthy of note is that the thermometer, plunged into the sand, indicated the boiling point, while the dust surrounded by this same sand and which naturally should have been as hot, was not, and remained constantly at some degrees lower than the boiling point for water. I therefore tried another method to attain my object. I placed the little dish by itself in a copper pan; I put the pan on a brisk fire and brought near the vessel some burning coals. Finally, the thermometer, the bulb of which was always buried in the dust, indicated the desired

degree of heat. But it cost me a part of the dust. The middle of the mass of dust could be brought to the boiling point but not without burning that portion nearest the sides of the container and reducing the burned dust to ashes. Fortunately, the portion of the contents within the dish remained unchanged.

I blackened some choice wheat with the dust that had sustained these different degrees of heat. That that had been subjected to 20 degrees only was used to inoculate the seed of the first rows of plot 75. That subjected to 40 degrees was used as inoculum for the 3 last rows.

The six rows of plot 76 were sown, part to seed inoculated with dust heated to 60 degrees, and part to that inoculated with dust heated to the boiling point.

The 18 succeeding plots were not sown until March, 1754. I must, however, follow the thread of my discourse and relate the details of what the plots contained.

In the 77th plot I sowed darnel seed blackened with dust from bunted wheat grains.

In the 78th I sowed clean spring wheat that had been washed in ordinary well water, somewhat warmed and poured over the grain.

In the 79th plot was sown spring wheat inoculated since the 12 of January, 1754, with the dust of bunted grains of wheat.

In the 80th I sowed some spring wheat, at first dusted with smut, like that of the preceding plot, then simply washed it carefully in well water: it will be necessary always to assume that it was washed, after it had lain a long time in the contagious dust, as a preliminary to its receiving the different treatments to which I subjected it. They are here presented in the exact order in which I used them and relatively per plot occupied by the seed on which treatment was employed. Spring wheat was employed in every instance. I shall abridge my account by citing only the plot and each treatment or preparation.

Plot 81, an aqueous solution of saltpeter and lime: The lime will be present in all subsequent treatments except those for plots 90, 93, and 94.

Plot 82, an aqueous solution of common salt.

Plot 83, a mixture of 1 part of gray potash solution and 1 part of ordinary water. The basis of this solution, as I

have already said, was a pound of potash and 2 pints of water reduced to three *demi-setiers* after having boiled a half hour and been filtered through gray paper. I had noticed, as I shall soon have occasion to observe, that the saltwort and pearl-ash leaches, reduced to 3 *demi-setiers* per pound of ashes, were too strong for the seed and affected the germ; I nevertheless did not wish to omit from the number of my treatments these caustic and penetrating agents; I took part of them for tempering their effect; I mixed water in different proportions with these solutions and sought the point where their action agreed with the delicacy of the seed.

Plot 84, a mixture of 1 part of water with the same gray potash solution and 3 parts of ordinary water.

Plot 85, a mixture of 1 part of the leachings from saltwort ashes and 2 parts of ordinary water.

Plot 86, a mixture of 1 part of the leachings from saltwort ashes and 4 parts of ordinary water.

Plot 87, 1 part of the leach from pearl ashes and 2 of ordinary water.

Plot 88, 1 part of the leach from pearl ashes and 4 parts of ordinary water.

Plot 89, 1 part of the leach from ashes of new wood and 1 of ordinary water.

Plot 90, a mixture of 1 part spirits of nitre and 9 parts of water.

Plot 91, a lotion consisting of putrefied human urine, applied warm.

Plot 92, a lotion of common wash water, applied warm.

The two succeeding plots were not in the range of those under consideration. They were a little isolated, although wholly within the same terrain. Some spring wheat, similar to that of the preceding plots, was sown after it had been given the following treatments:

Plot 93, a mixture of 1 part of white potash solution with 2 of water.

Plot 94, 1 part of white potash solution and 4 parts of water.

It may be recalled that plots 37, 38, 39, and 40 of 1752 seed were excellent, well filled, and, so far as one could observe, showed only a trace of disease, as a result of the remedies applied with such striking success to the bunt-

contaminated seed consigned to these plots. Before they were reaped, I, myself, collected 3 or 4 bouquets of very choice heads. They gave me as plump and sound seed as I could desire. It was employed as such and without preparation alongside the first series of my plots in this border two feet wide and extending the entire length of the field I had devoted to this purpose.

The experiments, which I increased in number and scope and without confining myself to ordinary practice, were carried on in a tract of ground abutting on that occupied by my plots and separated from it only by a mere road. The seed employed in the one was almost the same as that sown in the other. That concerning which there was any question came from the vineyard in 1752; it was uprooted at the beginning of the following year. The owner of the piece sowed it to barley and immediately after the harvest the field was turned over to me.

Hardly had I taken possession before I had it plowed and gave it its first working. It was given a second during the course of September; a third some time after; and a fourth, toward the end of October, was the one that made it ready for seeding.

This piece of ground seemed to me good enough and, having been manured for a long time to keep up the vigor of the young vines grown there, I applied no sort of fertilizer to the soil. I had the satisfaction of finding the manure that had been applied thoroughly rotted and I was, therefore, free from exposing myself to the use of any suspected materials. It was situated between some vines, but very well exposed: 421 feet long by 38 feet wide. *When it was

* Figure 1. — — — — —

worked the third time, I divided it lengthwise into 2 equal parts. With a plow a furrow was made from one end to the other and this formed a sort of alleyway between the 2 halves of the field and as a guide to prevent mixing of the different kinds of seed consigned to them.

Each half of the ground was subdivided into 5 portions. The 1st and the 5th were 60 feet long.

The 2nd and the 4th were 40 feet long, and the 3rd was 221 long by 19 wide.

In the 1st, the 3rd, and the 5th divisions of one of the

*Figure 1.

halves, which will be designated by *A*, I sowed clean, non-treated seed. I had selected it from several bundles that had been furnished me by a farmer whose seed bore some reputation. This wheat was exceedingly choice, but mixed with it were still some bunt balls that had escaped the flail.

I employed this same wheat in the 2nd division after having blackened it with the dust of bunt balls.

I moistened with saltpeter solution and dusted with lime a portion of the inoculated seed and sowed it in the 4th division.

For section *A* of this tract I had chosen, as will be seen, the best seed as much to sow it in this condition as to use it inoculated with smut or treated after inoculation. As for the wheat designed for the other part of the same tract, which will be designated *B*, my purpose, on the contrary, was to seek out the most infected seed from the bundles and without having to inoculate it myself, sow a part of it in this condition and apply to the rest of it some remedies that would protect it against injury.

The harvest of 1753 had produced for me too many bunt-infested sheaves; so many, indeed, that it was not easy for me to find wheat from the flail, or even from the fan that was not still covered with the black dust of diseased grain.

What I employed in part *B* will be understood when I say that the workman who threshed the infected sheaves where I gathered them, and who fanned them, saw the black dust from the sheaves rise like a dense cloud during the action of the fan, so that he was himself covered and frequently forced to cough, so much was he affected. I made use of this grain, thus smutted, and without any previous treatment, in the second division of part *B*.

For the 1st, 3rd, and 5th divisions the seed was prepared as follows: I put it in a tubful of water and washed it. I poured off this first water, which looked like ink. I poured some clean water into the tub; I stirred the grain and poured the water off a second time. I repeated this same operation as many times as it seemed necessary in order that the grain might be clean and of excellent color and that the water might run somewhat clear from the tub. The seed thus washed was exposed to the sun that it might dry. Later, I washed it in ordinary lukewarm wash water and

put it out to dry a second time. Then I dampened it with 3 pints of water containing a pound of lixiviated potash: the same quantity of similar potash had given 8 ounces of regular salt. I dusted the seed, still moist, with quick lime, stirring it well at the same time in order that the lime might reach every part of each seed; and when it was dry I sowed it in the 3 divisions already referred to.

The quantity of seed I thus treated could go into a bushel measure and weighed 34 or 35 lbs. The three pints of wash water was more than enough to moisten it.

In the 4th division I employed the same kind of inoculated seed, but I first freed it of that which rendered it dangerous. I first washed it in ordinary water and then in common wash water; I dampened it then with an aqueous solution of saltpeter and sprinkled it with lime.

The day on which I sowed this little patch of ground I, myself, supervised the planting of the wheat in each division and saw to it that it was sown exactly in the order I had arranged. I saw, in the first place, that all the wheat, clean as well as treated, was sown; and then that that had been dusted with smut. It was necessary, in distributing the wheat, which I tried to guarantee against all contagion, that the hand of the sower be clean and that it be free from the infectious dust of the bunted kernels.

When the ground was covered with seed, and a moment before the plow started, I set stakes at each end of the furrows that had been traced so as to clearly mark out the divisions that had helped the sower to avoid throwing seed beyond its prescribed bounds.

After the soil had been worked down, a cord was extended from stake to stake: the furrows, which the plow had filled up, were retraced and thereby I saw remade the same plot of ground I had originally laid out.

I have dwelt somewhat fully on the preparatory steps for this experiment because it fully came up to my expectations and has seemed to me worthy of attention.

I made it conform to the ordinary method of cultivating the soil. As a consequence, it will impress the farmer who, yielding but little to reason, desires the obvious things, the customary things, the facts clearly analogous to those that pass perpetually before his eyes.

If I had limited myself to a procedure of certain pre-

cautions and satisfied the eye by the symmetry and regularity characteristic of my plots, it would have been thought by some farmers that the success of this project was due principally to my care; and that, had it been conducted in the open country, it perhaps would not have been followed by such happy results as I reported.

This reasoning, easily comprehensible though it may be, will not be accepted even today.

I took for my large-scale experiment the first piece of ground at hand. I left it much as I found it, so far as concerned the fertilizers that had been applied to it; I plowed it; I sowed the seed according to custom and at the same rate as the farmers sow theirs. Thus far my practices and theirs agreed: most of our operations were the same: if there was any difference in our procedure it was only in relation to the preparation of the seed.

All of the farmers were then forced to agree that a man, in copying them, that one of their own number, if you please, proved that the dust of the kernels of diseased wheat was the true source of wheat smut and that to protect them against this disease there are certain remedies.

I feel that the reader is fatigued with having seen up to this point only a recital of a large number of experiments and that he must be awaiting impatiently the results obtained from them.

I shall present them by adhering to the order I followed from the beginning and by going back to the first sowing I made.

The details pertaining to the home experiments will follow immediately after the results from the piece of ground worked with the plow, and they will give added certitude to the interesting facts observed.

We are already informed that the little parcel of ground where, in September, I had put in 14 rows of wheat and 2 rows of darnel grass did not turn out as I had expected. I had fancied that in sowing some of the smut-contaminated seed at intervals of 7 or 8 days I would be able to detect different shades of color, even in the very beginnings of the disease in the wheat, and to follow the progress of the contagion, up to a certain point. I was deceived. The last row, sown to blackened seed, seemed to me like the second, where, also, the seed had been inoculated: It was plain that

I could consider the plants only from the standpoint of their more or less languishing condition.

I could not perceive the progress of the disease as I had hoped to do. There was an easily detectable difference between the healthy and infected wheat plants when they emerged and put out their first leaves, and especially when they were far-advanced; but, among the infected plants, themselves, the degrees of frailty were not perceptible, at least not so to me.

The first 6 rows of this little piece were perfectly beautiful before winter set in. The wheat plants were very vigorous; some of them had thrown out 4 or 5 tillers and resembled plants of rye: but the rigorous cold to which they were subject injured them not a little. A great part of them perished; the plants that were marked to bear smut heads nearly all died. I was scarcely able to find one of these latter in the rows that had been sown to bunted seed. Moreover, the other rows, the darnel as well as wheat, were in ordinary condition and produced about what I expected, that is to say, the majority of heads bunted wherever the seed had been inoculated and were very healthy in those plants grown from clean, carefully selected seed.

From the last observation I made it would appear (1) that there is some wisdom in not sowing wheat too early, at least in certain climates; for the severe frosts are capable of killing plants that have much vigor and whose stems already show evident growth. On the other hand, plants that have hardly more than emerged resist these freezes and escape, underground, from any harmful effect from external cold, which the tillers of too-far-advanced plants apparently cannot withstand.

(2) That the bunt-infected plants are more apt to die during rigorous winters than are healthy, well-formed plants. These people believe that, in some way, severe remedies weaken the plants in which a baleful leaven still resides, and, as a consequence, a serious disease nearly always proves fatal.

I shall not dwell on this striking alternative of healthy and bunted heads, yielded by the ten rows of the little piece as having any relation to the seed sown there. This and the certainty of a virus in the dust of the bunted kernels of wheat, as well as darnel, and the very real advantage of

sowing only sound seed have been demonstrated beyond question.

From the condition of the first 20 plots, where manures of different kinds were applied, it has lately been decided that suspected straw, mixed with the excrement of animals, contained no longer anything dangerous to the seed.

It has not been overlooked that each kind of fertilizer had been applied to 4 plots of which the first two always contained clean, noncontaminated straw and the other two, suspected straw. There was not a bunted head in plots 1, 2, 3, and 4, where cow manure had been applied. I saw only one in the 3rd of the 4 plots where sheep manure had been used; I saw none at all in the remaining three. There was none in the 13th, 14th, 15th, and 16th plots into which dry sheep manure had been worked; none, in fact, in the 4 following plots where I had employed sheep manure.

The 5th, 6th, 7th, and 8th plots contained horse manure. I noticed 6 bunted heads in the first two and 10 in the other two. Since the plots into which clean straw had been worked bore 2 or 3 diseased plants, it is necessary to refer back to the seed to find the source of the slight infection and to assume that if bunt is manifest in some heads in the two plots containing suspected straw this is not a result to be attributed to these lots of straw but rather to some contaminated seed mixed in with the seed sown in these plots.

It has been noted in the description of the experiments of 1752-53 that the 19th and 20th plots produced a great many bunted heads. I attributed the cause to suspected straw, which did not lose its contagious principle when worked into the two plots containing sheep manure.

As we shall see, this was not the case in 1753-54: Not a diseased head could be found in the 15th and 16th plots, although they contained as much sheep manure and suspected straw. It is true that the dry manure of which I made use the second time was not precisely of the same quality as that employed in the first instance. A great quantity of hay did not underlie it as it did in the other lot.

Being unable to find some that was absolutely like it, I used that which differed the least. The latter was, even at best, only the dried droppings of the animal: without doubt it served the purpose of the dung of other animals in relation to the suspected straw and was an excellent detergent.

There were in these 20 plots some heads of smut [loose smut], which were not traceable to one sort of fertilizer rather than another nor to suspected rather than clean straw.

Four or 5 heads affected with ergot were found, as many in the first three rows of plot 21, sown with clean rye, as in the last 3 in which seed previously contaminated with the dust of powdered ergot had been sown: further proof of the fact that this dust carries no contagion for the seed.

In spite of the cold weather (approximately 15° below zero, Reaumur) of February 2, 1754, I saved several of the little grubs, or, to better express myself, the little caterpillars I had found in some of the rye ergot grains on which they had fed and concerning which I have already spoken. Four of them metamorphosed into quite pretty butterflies, the wings, legs, and antennae of which were studded with white spots. Others were similarly covered with dark musk spots.

These butterflies were of the smallest sort and seemed to me to be quite common. I have seen their like, or I am much deceived, on the surface of water, exposed to the sun, in a washtub—water to be used later for garden irrigation.

I do not recall ever having seen them in the open country: it is none the less true, however, that butterflies of this kind may have oviposited and pupated there, for I am wholly ignorant of the special technique by which they protect their offspring and prepare food for them in the very retreat within which they develop. It is none the less true, I say, that they may have attached their eggs to some grains of rye out of which came the little caterpillars that I raised; that these grains, changed into ergots through some kind of derangement in their structure, served as food for the caterpillars; that the latter metamorphosed into butterflies; and that these insects became in turn the cause of several ergots in laboring for the conservation of their progeny.

I have every reason to believe that the ergot commences to form through an oozing of the juice contained in the kernel altered by the insect. One of the plots, to be considered when I report on the home experiments, was darnel grass. In examining it one day, I noticed a kernel of this kind that was much larger than the others and partly covered with a honey-like liquid. I marked the head containing

it and observed it from time to time. The unctuous liquid which adhered to it, gradually dried, the kernel enlarged, elongated a little, became black, and changed into a real ergot.

Either a large number of rye kernels, merely altered by the insect, become converted into ergots, contain no caterpillars because of absence of eggs or a large number of eggs perish because of divers untoward circumstances. I gathered only the small lot of ergots that harbored the caterpillars which I reared after having rejected many others in which there appeared to be no vestige of an insect. And, this year, out of 200 ergots, or thereabouts, I found but four that contained caterpillars. They made themselves known in a rather striking manner. The end of the ergot enclosed by the glumes and where, without doubt, the egg hatches is ordinarily pulpy, slightly moist, and apt to become spoiled. A little hole could be seen there that is the entryway of a sort of gallery that runs the length of the ergot and that the insect made in the process of feeding itself. One nearly always sees on the epidermis of the ergot a tiny opening that communicates with the gallery and by which the caterpillar rejects a portion of the material it devours. This residue may be compared to the fine dust one observes on wood tunneled by worms in the little channels where the insects are lodged.

Sometimes a caterpillar entirely consumes the ergot in which it is concealed leaving the outer covering reduced to dust. Last summer I put an ergoted grain of wheat in a small glass goblet and my intention was solely to keep it there because it is very rarely one finds ergot in wheat. This ergot, by chance, contained a caterpillar. It had lived there; the ergot was entirely consumed; the rind reduced to dust, conceals the insect today and there it will remain, no doubt, until time for its metamorphosis.

It is observed that there are years when ergot is very common, others when there is very little, and that the rye of certain countries is more subject to this disease than is that of others. The optimum time for the propagation of the insects that cause it, the divers circumstances that may cause their death, the great number of these little animals, once spread abroad in a country and proportionately multiplying there, seems to me to be a natural explanation of

these differences; as, in the case of another sort of insect, they clearly explain why our fruit trees are sometimes severely damaged, sometimes quite free from damage; and why, in some cantons, a reasonable quantity of fruit is rarely harvested.

The dust produced by the grains of bunted wheat certainly possesses no contagious principle for rye. Some rye was bunt-dusted before being sown in plot 22; not a head, however, gave the least indication of bunt.

This same dust has no virulence whatever for 4-row barley. The 3 last rows of plot 23 and all 6 rows of the 24th, the seed for which had been smutted bore heads as sound and healthy as those of the 3 first rows of the 23rd plot in which clean, bunt-free seed was sown.

What is more, 4-row barley, according to my observation, seemed subject neither to bunt nor smut nor ergot. For several years I noticed no trace of these 3 diseases on 4-row barley. Rust sometimes attacks it.

The 8 plots devoted to variously treated darnel-grass seed never threw any light on the efficacy of the remedies I employed for their protection against bunt. The severe freezes killed more than three-fourths of the plants and the loss was especially marked among the diseased plants. We have several times observed that the dust of bunted seeds of darnel was fatal to darnel. The seed of the three last rows of plots 25 and 26 had been dusted with this kind of smut and should have produced a large number of diseased heads.

Nevertheless, in the number of plants yielded by those 6 rows I was able to find scarcely one that bore a diseased head.

One may easily conceive that, for the time, the 3 rows of the same 2 plots, where I had sown clean darnel seed, where the seed had been subjected to different treatments, there must have been no bunt. I searched for it without success.

From the condition of the plots sown with treated seed one can draw no conclusion as to whether or not the remedies used were effective, for the diseased plants, if there were any, had winter-killed and there was nothing left for examination.

One may, it is true, presume from the advantageous effect which the same remedies produce on wheat that they

were favorable for the darnel and prevented the infective dust with which the seed had been contaminated from becoming a principle of contagion. But I wish to state as a certainty only that that is revealed in the light of repeated experiments and has become decisive through satisfying the objective for which they were designed.

There comes to mind here a matter for consideration. It has been observed that in certain years the darnel is very abundant among the rye plants and that sometimes very little can be found. It is known that this grass very largely succumbs to the rigors of winter. It would be quite natural to believe that where darnel is common everything has favored its growth and that, on the contrary, it has been overtaken by untoward circumstances when it is scarce. This idea would appear reasonable and ought not to offend one's good sense, like that of certain husbandmen who believe that rye sometimes changes to darnel and that there are years when this transmutation is more favored than in others by the weather conditions. I shall not give serious consideration to this grotesque idea. It will suffice here to guarantee to the farmers that they harvest only rye to the end that the seed they use be free from darnel and to assure them that the abundance of this weed is a result of their carelessness. I tell them this, and I repeat it, as an absolute fact, that I have never harvested darnel at any time when I have sown nothing but pure rye; and that I have never found any rye where and when I have sown only darnel.

In the eight plots under consideration the darnel plants that had survived the cold were few in number, but strong and vigorous. They had thrown out several culms whence developed heads that were long and laden with grain. The rows were much better filled than I had dared to hope for, so much had these eight plots at first seemed destitute of darnel plants. It is true that in the spring I had taken the precaution to eradicate all the weeds that had grown in abundance among the darnel plants and I took particular care to see that the few remaining ones were in no wise damaged by the vigorous vegetative growth of the large number of plants I had lost.

This, again, is proof of the advantages that result from the new method of cultivating the soil. A few seeds well distributed develop without injuring each other; the plants

can easily extend (have room in which to grow); the air impinging on all sides strengthens them; and the heads, abundantly provided for, yield a large quantity of seed.

There perhaps had been some hesitancy in believing that the suspected straw was dangerous for the seed, seeing that nearly all the first plots, where this straw had been employed as part of the manure, contained not a single diseased head and there were only a few where any were found. But, in addition to what has already been said, that the suspected straw is harmful to seed in its near vicinity and that one cannot use it with safety in so much as it is mixed with the dung of animals; it will be necessary in any event to yield on this point when I shall assert that plots 34, 35, and 36 where, it will be recalled, I had placed chopped contaminated straw, spreading it in the furrows, and mixing with it a little of the soil in order to facilitate germination of the seed; when, I say, I shall assert that those 3 plots contained many bunted heads. It is impossible to go so far back as the seed sown on this straw in order to find the cause of the trouble; this seed was the same as that I used in plot 33, into which had been worked some healthy straw and wherein I noticed 3 diseased heads; it was the same as that sown in plots 39 and 40, where I observed not a single bunted head. It is true that the disease was more pronounced in plots 34 and 35 than in plot 36; but it also is true that the contaminated straw, spread in this latter plot, was absolutely free from heads. I had, however, left them in the straw used in the two other plots. And here is new proof that the contaminated straw had something pestilential about it, either that it actually was so or that it had every appearance of being so, since it acquired a malignity only through accidental contamination with bunt dust when the bundles were threshed. For, in removing from it the bunted heads I eliminated from this straw the principal source of its banefulness and rendered it less injurious to the grain.

I am now certain that miracle wheat is subject to bunt, but decidedly less so than other varieties. I found only a few bunted heads in plots 37 and 38 and in the dozen rows of this wheat that I had sown separately. The seed sown in these two plots and in another isolated spot had been dusted but with dust from the bunt balls of ordinary wheat, and I

had neglected nothing in an effort to insure firm adhesion. Plot 37, as well as 38, was sown on the 24th of October. I do not know why the miracle wheat failed to come up. I presume it was too deeply planted; in fact, I recall that this plot was a little lower than the others; that the soil had unwittingly been removed from it in order to reduce it to the level of the neighboring plots when the wheat was sown, and that thereby a stratum of soil was uncovered that was too stiff and through which the wheat plants were unable to force their way. I reseeded this plot 37 in the month of March, using miracle wheat infested with bunt, like that sown in the first instance. It came up nicely, threw out a number of fine tillers, and soon attained the height of the wheat in the neighboring plot. This experiment seemed to promise that, given a good seed bed and everything else favorable, one may successfully sow miracle wheat in March and have it come to maturity.

I have already stated that plots 39 and 40 contained not a single head of bunt. The seed to which they were sown had come from choice heads, I, myself, had gathered from standing plants at the time of harvest. In one of these plots clean seed had been sown and without any previous treatment. In the other the seed had been washed in a potash solution and then dusted with lime. The perfectly healthy condition of these two plots demonstrated clearly that when the seed is clean every principle of the disease is banished, and that treatments would be superfluous if it were possible to obtain seed that had not come in touch with anything contagious. But, unfortunately, in a great many very beautiful heads that had been gathered with care there was sometimes found a diseased kernel—a partly sound, partly bunted one. This kernel escaped detection; mixed with the others, it contaminated them, soiled them with bunt dust; or if one part only of this seed is blemished, the germ not being at all affected, it is the progenitor of a diseased plant, it transmits the virus more decisively than it had, itself, received it and passes it on in full to its posterity. This reflection, which it is but natural to make here, will have its application when I speak of plot 69.

Let us recall for a moment what has been said, in the general exposition of my sowings, on the subject of the kind of seed sown in plot 41. The bundles whence I had col-

lected this seed were in great part infected. It was, itself, covered with dust from the bunt balls and was intermixed with many other and diseased grains, which the flail had not crushed. It was from this small lot of wheat, so capable of perpetuating infection, that I first drew a portion, such as it was, for plot 41. It should here be mentioned what would have transpired in the other 23 plots, if the seed sown in them, and which I drew from the little pile of infected grain, had not been insured against contagion by excellent preservatives. Of the 2,700 to 2,800[2] heads in plot 41, at least a third of them showed bunt. Such were the very marked results of the contamination of the seed sown in this plot. Such they were, moreover, wherever this seed was sown without disinfection; such, in fact, is the sort of evidence we must keep before us in going over the plots designed to show the effect of remedies if we are successfully to decide their efficacy.

I was satisfied simply to wash, but carefully, in ordinary water the same infested seed I had sown in plot 42. This precautionary measure was not sufficient to completely eliminate the cause of disease.

I noticed some diseased heads in this plot. Aside from this it was in very good condition. The mere washing in ordinary water was consistently given the seed sown in the 22 following plots before subjecting it to the various treatments.

One of the better methods of treatment, and one that causes no injury, is to wash the seed in ordinary wash water. Only one bunty head was found in plot 43.

Lime water, almost boiling hot, poured over the seed sown in plot 44, had a marked effect and, in general, should be considered advantageous.

The seed sown in plot 45 had been washed in ordinary wash water and dusted with lime. Nevertheless, there were 6 bunted heads. We shall see no more of them in the 11 plots I am about to refer to; I did not discover a single diseased plant in them, so completely successful was the remedy. It was due to the lye from new-wood ashes in plot 46. It has not been forgotten that the seed was dusted

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[2] It will be necessary to assume nearly the same number of heads in each plot; as many sound as diseased.

with lime immediately after it had been dampened by the water of the different washes.

In plots 47 and 48 success was due to the lye from pearl ashes; that of plots 49 and 50, to the lye from saltwort; that of plots 51 and 52 to potash solution; that of 53 and 54 to a saturated solution of sea salt; and that of 55 and 56 to a saturated solution of saltpeter. There is a very real satisfaction in comparing with these 11 plots the 41st plot; to see the one for the most part full of diseased heads, and the others containing nothing but sound heads; and to recall that for the 12 plots the seed of each had been taken from the same infested lot, and to realize that the several treatments were alone responsible for the difference. The success realized through the application of some other remedies was nearly as complete as that just referred to.

The putrefied cow urine, employed as a treatment of the seed sown in plot 57, reduced the amount of bunt to a single head. With the same sort of urine, combined with powdered quick lime, there were but 2 heads of bunt in the 58th plot. In the 59th plot, the seed of which was treated with putrefied human urine, there were only 3 bunted heads. The 60th plot, treated with the same kind of urine, combined with powdered quick lime, produced but 9 bunted heads. The 61st plot, the seed of which was treated with liquid manure, contained only 3 bunted heads. Plot 62, treated with liquid manure combined with powdered quick lime produced but 2 bunted heads. With a mixture of spirits of nitre and river water in the ratio of one to nine, plot 63 yielded only one diseased head. Plot 64 was sown to seed treated with the following mixture: ⅛ spirits of nitre, ⅞ river water, and pulverized lime. But six heads of bunt were found in this plot.

From the foregoing it is obvious that the treatment involving the application of powdered quick lime to seed dampened with putrefied human urine was not quite so effective as was the application of urine alone. Let us note again that spirits of nitre without the addition of lime proved more effective, even though it was weakened by the added water, than did the mixture employed in treating the seed of plot 64. This alkaline substance probably too quickly combined with the spirits of nitre and before it had had time to produce its prophylactic effect.

The seed used in sowing the 4 succeeding plots was from the 1752 crop and came from the same bag. In plot 65 I sowed clean, nontreated seed. It produced not a single bunted head.

There was every indication that the wheat that had been left covered with bunt dust for a year acquired no more susceptibility to the disease and became no better adapted to spreading the contagion than if it had been covered with bunt a few days before seeding. Plot 66, for which I had used seed that had been buried in bunt one year, produced about a third of a crop of bunted heads. This is a commonly experienced loss and the disease is sometimes more pronounced in places where the seed has been sown soon after inoculating with smut.

In superficially washing this infested seed, in dampening it afterward with potash solution, and dusting it with lime, I removed from it nearly all of the contagious principle. I found only 3 heads of bunt in the 67th plot.

The saturated solution of saltpeter with which I moistened the seed for plot 68, combined with the quick lime, which I applied immediately after, proved a sovereign remedy. Plot 68, where this seed was sown, contained not a single bunted head; at any rate, I noticed none there.

Such, again, is the spectacle we have several times delighted in and which is always agreeable to observe. There, some clean, carefully selected seed which yields none but healthy heads. Here, the same seed, infested with bunt, is a well-spring of diseased heads; whilst, on the other hand, this same seed loses through a treatment that which makes it injurious and causes it to produce only sound, healthy heads.

In plot 69 some bunted heads were found, although the seed sown in that plot was apparently very clean and had been selected from heads I had carefully collected.

Now is the occasion to apply what has been stated in the foregoing. A seed affected with bunt may be encountered in a head otherwise sound enough to escape detection of disease. This bunted kernel might be crushed in threshing the head and become the source of disease in good seed smeared with the infective dust. A partly smutted, partly healthy, kernel has been found in a head of excellent appearance and has slipped into the seed used for sowing only to become the source of a diseased plant.

The same élite wheat had been contaminated with the dust of bunted darnel seed and sown in this condition in the 70th plot. Two-thirds of the heads produced were bunted. The experiment repeated showed again that the dust of bunted grains of darnel is at least as dangerous for wheat as is that of its own bunt.

The bunt disappeared entirely in the 71st plot. Every plant without exception was healthy. Potash solution and powdered lime had removed from the seed sown in this plot the external principle of the virus. Without this treatment, it would have been the source of a disease similar to that we observed in plot 70, since, before receiving the treatment, it had been inoculated with the dust of bunted darnel seeds.

The saturated solution of saltpeter, without question, is one of the best preservatives I have noted. Not a single diseased head could be found in plot 72. The dust from the diseased darnel kernels was, in spite of its pathogenicity, without effect on the wheat sown in that plot.

There yet remains for us to give special consideration to the different and very marked results obtained, not only from the standpoint of the soil, but also with respect to the condition of the seed employed. Sometimes, used in all its purity, it produces nothing but sound heads; at other times, covered with an apparently innocuous dust, it becomes an abundant source of disease; or, in this latter condition, which may result disastrously, it is the cause of no trouble whatever because it has been subjected to a simple and apparently superficial treatment that protected it against the disease principle.

That trail of smut, which I laid in each furrow of plot 73, half-an-inch from the seed, proved as disastrous as it did in 1753. At least a third of the heads produced in this plot were found bunted. This seed was pure, however, and carefully selected. Employed in this condition of purity, it had yielded in several places none but sound, healthy heads. Moreover, the precautions I observed will be recalled: The care I took to prevent contact between the seed and the trail of smut and to prevent the young plants from touching it until their first roots came in contact with it.

Is it not natural to fancy these same roots, still delicate and susceptible to the slightest impression, contracting the contagious poison of the smut through the development of

their rootlets in the path of the smut? Is it not natural to picture these rootlets as having been the initial avenue of the virus? Is it not natural to imagine that they transmitted it to the nascent stem and thence, subsequently, to the embryos enclosed within the head?

If a trail of smut, neatly laid down at some distance from the seed, was harmful to it, it is not surprising that this same smut, spread in the furrows of plot 74, where I sowed some seed, should have been dangerous and should have infected that seed before it had developed any plants. The majority of the heads in this plot were bunted and were indistinctly visible in all the rows. I made this latter observation because the seed I used for the first 3 rows was clean, though nontreated. That of the last two I treated with a saturated solution of saltpeter and then dusted it with lime before sowing. This would seem to prove that if, in the last two rows, half of the heads were diseased in spite of the seed treatment, it was because the infective principle had reached the young roots in their ramifying across the smut spread in the furrow bottoms and sufficiently abundant to cause a general infection.

The different degrees of heat to which I had subjected the bunt dust employed in smutting the seed sown in plots 75 and 76 rendered it only the more active in infecting the seed and to a degree quite proportionate to the degree of heat employed. Three-fourths of the heads in plot 76 were bunted, where, I stated, one part of the seed sown had been smutted with bunt dust that had been subjected to a temperature of 60°, and another part with the same dust that I had had so much difficulty in bringing to the boiling point. There were fewer bunted heads in plot 75. The dust with which I inoculated the seed sown in the 3 first rows of this plot had been subjected to but 20° temperature and that used as inoculum for the seed of the 3 last rows was heated to 40°.

It is by all means necessary to abandon the hypothesis that the dust of kernels of bunted wheat be thought of in the light of an ant hill of eggs that, on hatching, produce a prodigious quantity of very small insects that one might suspect of being the originators of the malady affecting the seed. A moderate heat such as 20° or 40° would have favored the birth of the insects, if indeed there had been

any. One would have been able to see them with the aid of a magnifier, and an appreciable and very characteristic damage would have been the result of their sojourn in the wheat head. I observed nothing of the kind. A great part of plot 75 was infected with smut, it is true; but, as in the other plots where there was bunt, the disease was present in the very midst of the plant; there the cause had been insidiously active; and I had seen only the outward evidences by which a deep-seated disease becomes manifest. A temperature of 60°, or one as high as that of boiling water, having been applied to the bunt dust, would infallibly have killed the eggs of insects, which we assume here to have been nothing other than the cause of this same dust, and, consequently, the disease would not have appeared in plot 76, since, according to the hypothesis, the insects from these eggs would alone have been able to produce it. However, we have observed that this same plot was one of the most severely infected and that three-fourths of the heads were found diseased.

The plots now about to be considered were sown in the first days of March, 1754.

The 77th plot contained darnel: I discovered there only one or two diseased heads, notwithstanding the fact that I had blackened the seed sown in this plot with dust from bunted wheat grains. Several extra rows of darnel that I had sown with seed left over from this same inoculated lot, gave me nothing but healthy heads such as one might have anticipated had one taken precautions against untoward circumstances.

What caprice! The dust of the grains of bunted darnel is fatal to darnel and to wheat. The dust from bunted kernels of wheat is pathogenic for wheat and innocuous for darnel. With what moderation must one reason from one fact to another, what analogy may one not suspect between them! All indications are that the two kinds of contagious dust should act reciprocally on the two kinds of seed, since one kind of dust is capable of this double action. All the experiments I have thus far made have been contrary to appearances and the darnel has never responded to the malignity of the wheat smut. This phenomenon raised my curiosity. I shall, this year, have opportunity to observe it and I shall

not fail to render an account of it in reporting the experiments on which I am at present engaged.

It is only too consistently true that, as I have indicated in my dissertation, spring wheat is more subject to bunt than is winter wheat. The material comprising my experiments in the 17 plots, the results of which I have yet to give, yielded some sheaves that I had threshed, leaving in them the small number of diseased heads they contained. Consequently, the seed was under suspicion and called for treatment. I washed in ordinary wash water the portion of this seed set aside for the 78th plot. It, nevertheless, produced several bunted heads. This wash, the reported use of which on winter wheat we have already noted and concerning the success of which we shall yet have opportunity to observe, was not wholly effective on spring wheat.

Let us consider how susceptible spring wheat is to the bunt-dust virus: Infected with smut it becomes the source of a disease that is nearly general in plot 79. In a total of 10 heads I was able to find scarcely one healthy one.

This same wheat not only becomes prey to the pestilential principle resident in smut, but it retains tenaciously a residuum of this principle, although by repeated washes one seemingly leaves in it nothing contagious. The condition of the 80th plot evidently confirms this, for I noticed in it many diseased heads. The seed sown in this plot had been allowed to lie smutted about 2 months. I had washed it well before sowing, but a part of the seed always retained the malignant principle. It required active remedies, those whose effectiveness already is known. Let us not forget that the bunt dust always was applied to seed such as this; that is to say, first inoculated and then washed in several waters.

I observed but one bunted head in plot 81. The saturated solution of saltpeter had produced its effect. The saturated solution of common salt had been less effective; for there were 6 diseased heads in the 82nd plot.

There were 7 bunted heads in the 83rd plot and an equal number in the 84th. The remedy applied to the seed of these 2 plots was a solution of commercial potash that I had more or less diluted in the fear that it might otherwise affect the seed. Concerning this, I have already spoken.

In the 85th plot I found 11 bunted heads and an equal

number in plot 86. The remedy applied in these 2 plots was a solution of saltwort lye. In each of plots 87 and 88 I found but 2 bunted heads. The leach of pearl ashes had exerted some action in these plots. I saw only one bunted head in the 89th plot. The leach of new-wood ashes was responsible for this result.

I noticed only a trace of the disease in the 90th plot, the seed of which had been treated with a mixture comprising one part of the spirits of nitre and nine parts of water.

Putrid human urine was by no means so effective a remedy for the spring wheat sown in the 91st plot as it was for the winter wheat and as we shall yet note it to have been in the small-scale experiments. This plot was not in such good condition as the preceding ones; several plants were affected with bunt.

I presume that the quick lime, with which the seed sown in the 92nd plot was dusted on removal from the ordinary wash water, was beneficial. With the exception of a single bunted head, all those produced by this seed showed no smut; while that employed in plot 78 and that had been merely washed in the same kind of water as that applied to the seed sown in plot 92 and had not been dusted with lime, did, as we have observed, produce several diseased heads.

The 93rd and 94th plots, both isolated, fully met my expectations. The wheat plants were vigorous, the individual tillers were tall, the heads filled with grain. They resembled the finest plots of winter wheat. I had, above all, the pleasure of finding only one head of bunt. The remedy employed in treating the seed of these 2 plots was an aqueous solution of caustic potash, more or less amended, and powdered quick lime.

Let us now return to the 79th plot, in which there was almost a maximum amount of the disease, since nearly one-tenth of the heads were affected with smut. Let us examine the effect of the somewhat less effective remedies on the spring wheat, which, compared with winter wheat, was always more satisfactory. Let us not forget that the same seed served as a basis for the different tests under consideration and that it differed from plot to plot only externally. Let us now observe the 2 plots before us and we shall boldly conclude that the dust of bunted grains of wheat is very viru-

lent for spring wheat. It is the recognized source of the disease and perhaps the only one in our clime; it certainly is the source of the greatest abundance of smutted heads. This dust, whether totally removed or arrested in its action, is no longer dangerous for the seed, howsoever inoculated, when subjected to the simple remedies I have indicated and that the farmer will find at his disposal.

The rows of wheat I had put in along the first series of my plots gave me some well-formed, healthy heads and among them I found very few containing any bunt. As is known, I sowed them to clean, nontreated seed obtained from a sowing of seed the purity of which was under suspicion and to which had been applied one of the best remedies without divesting it of the smut with which it had been inoculated. The result was that I found the wheat of those rows, the inoculated seed of which had been treated, in a healthy, sound condition. It had been radically protected against bunt and in it there remained nothing of the pernicious principle to be passed on to subsequent crops. That one's success is not always so satisfactory as it should be when art comes to man's assistance in this insidious, troublesome disease which calls for active, penetrating medicaments that do not always prevent under conditions of faulty application, is sometimes made obvious by external symptoms or by the unhealthy appearance of those plants one has brought forth.

In presenting the distribution of my plots, and my different seedings, I have neglected to say that at the end of the border I am now about to consider I had reserved a little space where I put in 20 rows of wheat. These rows were sown at right angles to the long axis of the border in order to avoid confusing them with the others that had been sown lengthwise. The seed of the first 5 rows was of clean wheat; that of the succeeding 5 was from the same wheat, blackened with the dust of bunt balls, that I had held in reserve one year. The seed of the next 5 had been inoculated with bunt dust obtained from only partly developed bunt balls: the head from which I obtained them had been collected on June 18, 1753; that is to say, about 5 weeks prior to harvest, when the smutty matter within the diseased kernels was soft and greasy. It was subsequently dried and I reduced it to a dust before using it. The seed of the 5 last rows was

inoculated with the dust from well-formed bunt balls, such as one obtains from thoroughly mature plants. There was not one bunted head in the first 5 rows. There were many in the remaining 15 and almost an equal amount in each.

Let us first note, then, that pure, clean seed comes only from sound healthy heads. Second, that from contaminated seed a great many bunted heads arise. Third, that smut, kept from one year to the next loses none of its virulency. Fourth, that this same smut, although imperfectly formed, is dangerous to the seed and already possesses the virulent principle that causes the malady.

In the order of succession in which I have presented my experiments and have given the results I am inclined at the moment to recall one of the most essential and give an exact account of it.

That to which I have here referred in relation to my plots relates to something that may have been lost sight of in what I said little farther back concerning the manner in which I laid out the ploughed ground and the condition of the seed I sowed there.

Let us take a casual glance at the project before looking into the condition of the wheat produced in this tract. The length of this little field was 421 feet and the width, 38 feet. It was divided lengthwise into 2 equal parts; these I designated A and B (Fig. 1). Each of these parts was subdivided into five portions, the first and last of which were 60 feet long and 19 feet wide. The 2nd and 4th were 40 feet long and the 3rd, 221.

The wheat sown in plot A was originally clean, carefully selected, but mixed later with some bunt balls. I sowed it in this condition, and without giving it any preliminary wash, in the 1st, 3rd, and 5th divisions. I inoculated with bunt the seed for the other two divisions also, but with this marked difference, namely, that the seed sown in the 2nd division was sown without treatment, while that in the 4th I moistened with a saturated solution of saltpeter without washing off the smut and then dusted it with lime.

The seed sown in plot B was originally all that was necessary as a source of disease. It came directly from under the flail, covered with smut as much so as though I had purposely inoculated it. I sowed it in this condition in the 2nd division.

For the 1st, 3rd, and 5th divisions it was washed in ordinary well water; then in wash water, and then in potash solution, after which it was dusted with lime.

Instead of the aqueous solution of potash, I used a saturated solution of saltpeter on the seed sown in the 4th division, after having washed it in ordinary well water and then in ordinary wash water.

Some bunted heads were found in the 1st, 3rd, and 5th divisions of part A. This should not surprise us. I stated that the seed sown in these divisions was mixed with some bunt balls. Their proximity was dangerous to the sown seed. As for the 2nd division, two-thirds of the heads were found diseased; moreover, the culms were shorter. I did not need to hunt for the two furrows that constituted the limits of this parcel of ground in order to set it off precisely from the 1st and 3rd divisions. One was at once struck by the backward condition of the wheat contained in it, by the bluish color of the heads, and by the shorter culms already alluded to. From the appearance of marked superiority of the culms, that bordered the first and third divisions alongside the second, one would have believed at first glance that the culms of the second division had grown in a slight depression of the terrain and that the level had suddenly recovered in spots where the wheat stood higher.

The saturated saltpeter solution followed by lime gave marvellous results in the fourth division. Almost no smut developed: We have observed the consistent success following this treatment.

I come now to part B, which was sown to seed thoroughly capable of perpetuating the disease. The treatment applied to the seed of the 1st, 3rd, and 5th divisions consisted of dipping it in an aqueous solution of potash and dusting with lime after giving it the customary washings in ordinary water and in wash water. Bunted heads in these three divisions were rare, much more so than in the corresponding three divisions in part A. The seed I used for these three divisions must, nevertheless, be regarded as very excellent and would have been accepted as seed by the exacting farmer. There are, then, some real advantages consequent upon certain treatments applied to seed; the success of these treatments is, therefore, quite favorable, since the most bunt-infested seed loses as a consequence whatever it

carries by way of contagion and produces thereafter much less of the disease than does élite seed, very satisfactory to the eye, but mixed, nevertheless, with bunt balls and not treated before sowing.

The disease was present in the 2nd division of part B. It was not observed in so many plants as in the corresponding division of part A. The reason for this is obvious. I had purposely and copiously smutted the seed sown in this latter division. The seed I used in the 2nd division of part B, although heavily inoculated with smut, carried less inoculum than that sown in the 2nd division of part A. I used it in such condition as I found it on leaving the fan, devoid in the main of the bunt dust that covered it as a result of the operation of the flail. This dust, being the source of the disease, produced a more or less marked effect according to the more or less abundant dose of smut received by the seed. The disease, I stated, prevailed in the 2nd division of part B: The considerable number of bunted heads in this division therefore indicated that a like infection surely would have occurred in the four other divisions but for the fact that helpful remedies had been applied to the seed that I had sown in them. Since it [the seed] was originally the same as that that produced such a large number of bunted heads, this second division was, therefore, the spot whence issued the pronounced ray of light on the veritable cause of the disease: it was this unsuspected evidence, that I have always made it a rule to allow to stand, when it is desired to test a remedy on some bunt-infested seed and remove from it every element of contagion.

There was scarcely a vestige of the disease in the 4th division of part B. The washings with ordinary [well] water and with wash water, which preceded the treatment with a saturated solution of saltpeter, followed by dusting with lime, seemed superfluous. The saltpeter solution, plus the lime, was in itself a sufficient preventive for the seed I sowed in the 4th division of part A. The bunted heads were quite as rare as they were in the corresponding division of part B, where this same treatment had been applied only after the seed had been given the two preliminary washings referred to in the foregoing. Nevertheless, these preliminary baths would no doubt be very useful in instances where a less active preventive agent than nitre solution and lime,

or some other that I have seen work with complete success, might be employed.

The large-scale experiments of which I am speaking carry conviction on the fundamental points that I have made my objective. Let us, however, compare with these experiments those I made on a small scale and which I followed each day because they were daily before me. If pictured in the ensemble they will together confirm what I have asserted, and the least reasonable doubt will no longer be entertained.

The little garden surrounded by buildings, where, in 1751 and 1752, the experiments were conducted of which I have already given the details, was used again in 1753. It will be recalled that it comprised two squares of unequal size, the dimensions of which have already been given.

I made, as in the preceding year, two plots in the first and four in the second square. Each of the plots of the first square contained six rows of wheat; there were but four in each plot of the second square. The wheat I sowed in each of the six plots, if we except the two last rows of the second square, where it was necessary to sow some clean seed on some prepared manure; this wheat, I say, was originally contaminated with smut and was the same as that that comprised the material for plot 41 and 23 others, the results of which I am here presenting.

The first row of Plot A (Fig. 2) of the first square was sown to this bunt-infested wheat; the 2nd row to some of the same seed merely washed in ordinary water. It will be necessary always to assume the seed to have been thus washed before treating it with the remedies that follow. The 3rd row was sown to the same seed dampened with ordinary wash water; the 4th, the same, moistened with potash solution; the 5th, the same, wetted with a saturated solution of common salt; and the 6th was sown to the same seed, moistened with a solution of saltpeter. The seed sown in the 4th, 5th, and 6th rows was dusted with lime after each treatment.

The first row of Plot B was sown to the same bunt-infested seed. The treatment materials employed on the seed for the five remaining rows were putrefied bovine urine for the 2nd; putrefied human urine for the 3rd; a mixture of one part of spirits of nitre to seven parts of water for the

4th; a solution of lye from the ashes of green wood for the 5th; and a solution of lye of pearl-ashes for the 6th. This seed also was dusted with lime following the three last treatments.

The first row of Plot A (Fig. 3) of the second square was sown to the same bunt-infested wheat. It was simply washed for the 2nd row. The remedies applied to the other two were common wash water and a mixture of one part of spirits of nitre and seven parts of water.

The remedies applied to the seed sown in Plot B were an aqueous solution of soda for the 1st row; lye of pearl-ashes for the 2nd; a saturated solution of common salt for the 3rd; and a saturated solution of saltpeter for the 4th.

The seed employed in the last row of Plot A and in the two first rows of Plot B was dusted with lime before sowing.

Plot C contained nothing but darnel grass. The seed of the first row was clean; inoculated with the dust of darnel smut for the 2nd; moistened in this condition with soda solution and dusted with lime for the 3rd; and dampened with saturated solution of saltpeter and dusted with lime for the 4th.

The wheat sown in row 1 of Plot D was washed with putrefied human urine; that of the 2nd row, with liquid sheep manure. That sown in the two last rows was clean: I sowed it on dry sheep manure combined either with clean or with suspected straw.

I shall not enter upon the details of the product of these two squares. One can see exactly what happened by examining the diagrams I have included at the end of this supplement and in the conclusion of which are the results of each plot. The crop I obtained from the home experiments was the result of my own work; nobody was with me while at work. According as I pulled up the wheat plants of a row I separated the sound heads from the bunted ones. I faithfully counted those of each lot and made a note of the result in referring them back to the particular rows from which they had come. If one cares to depend upon my accuracy and accept the results I have furnished, he will have the satisfaction of seeing how that same seed which, in the 1st row of Plot B (Fig. 2) had been the source of 639 heads of bunt out of a total of 1,266, and had produced only healthy heads in the 4th and 6th rows of Plot A and in the

3rd and 6th rows of Plot B of the same square. It will be noticed, moreover, with satisfaction that putrefied human urine, that detergent so simple and always at hand to whomsoever wishes to use it, served as an excellent remedy, since, in 1,053 heads produced by seed originally inoculated with bunt, there was not to be found a single head of bunt. One will note in examining the contents of the second rows of Plots A of the 1st and 2nd squares that the washings with ordinary water and several times repeated did not suffice to remove all of the contagious principle from the seed, for in one of the two rows there was yet to be found 15 bunted heads. One should not fail to observe how disastrous to darnel was the dust of its own smut, since of the 369 heads produced in the 2nd row of Plot C of the 2nd square 276 were stricken with the disease, while they were all free from it in the 1st and 3rd either as a natural result of the purity of the seed or as the result of the remedy applied.

Confirmation of the accuracy of what I have said will be apparent where use was made of those manures with which suspected straw was included provided the latter was so buried as to lose whatever of contagion it bore.

It will at length be recognized that the small-scale experiments agree with those carried out on a large scale, that the dust of the bunt balls is everywhere a pest when one does not counter it with something that will nullify its action, and that always it ceases to menace the seed when treated with certain preventive preparations.

It should be noted in considering the product of each row that those wherein the seed had been treated with a solution of soda or with pearl-ash lye, and particularly the 4th row of Plot A of the first square, yielded a much smaller number of heads than the neighboring rows, although in each of all the rows the same amount of seed had been sown. One must conclude that these solutions[3] were too severe on the seed; that they attacked the germ and killed part of the seed to which I applied them. There will be no doubt about that when I state that, in order to be sure that the different remedies I had used did not threaten the germinability of the wheat, I placed in orderly fashion on a piece of water-

[3] They were the product of 1 pound of ashes and of 2 pints of water reduced to 3 half-septiers after having been boiled and filtered.

soaked flannel some kernels of each of the lots of wheat I had treated and that I followed their thus exposed germination. Several kernels that had passed through the soda solution and the pearl-ash lye swelled extraordinarily; they took on a reddish color; the milky juice with which they were filled hardened a little and became like softened chalk; the pericarp was ruptured; none of the kernels thus affected germinated. We are, however, not denying the fact that these solutions were of some value. Diluted to a certain strength they caused no injury to the spring wheat, delicate as it was, and, by exercising certain precautions, excellent protective results were obtained. Although I presume little use will be made of diluted spirits of nitre as an agent for the treatment of bunt-infested seed, I must, nevertheless, digress to state that this remedy is likewise very active and that I have perceived, especially in relation to spring wheat, that there is something unusually penetrating about it.

The small-scale experiments, the results of which we shall now consider, are sufficient, I believe, to prove undeniably the facts they were designed to adduce; I believe they suffice to present more convincingly the facts of the same order derived from the large-scale experiments. I shall, therefore, abstain from reporting others, which I conducted either in clean moss or in small isolated areas, always with the intent of establishing the origin of the disease, or to prevent it by means of treatments. They would add nothing to what has already been presented here, and would take in several other experiments that, perhaps, have already become too numerous. However, one must make certain allowances here, as well as for the lengthy detailed account I have given pertaining to the matter I have treated, out of consideration for the popular benefit that has been my objective. It was felt that it was a matter of destroying universal prejudices, of determining the cause of a malady that, in an average year, takes, in France, a total amounting to one-twelfth[4] of our crop, of conducting

[4] There are some cantons near Troyes where the bunt of wheat is a general and common disease. M. de Bellenglise, Captain of Cavalry, wrote me from the neighborhood St. Quentin, where his estate is situated, that this malady causes much damage there. M. Crollat, procurator to the King in l'Election à Gannat, has called my attention to the fact that where he resides, and which constitutes a part of Limagne d'Auvergne, a delightful country and very fertile for wheat, the disease has been prevalent for at least 50 years. It was noted at the beginning of my dissertation, that in several Provinces there was complaint that the wheat was suffering from bunt.

some experiments, and of repeating them often enough so that the reader, weary with seeing this same cause of destruction reproduce itself without ceasing, under his very eyes; it was, finally, a matter of trying a large number of remedies, assuring myself of their merits, making them easy of execution, of taking into special account their economy of application, and of winning the confidence of the farmer by showing him that attention necessary to spare him from certain and inconsequential domestic outlay in order that he might obtain a general and important benefit.

This concern for the interests of the farmers occupied me very especially in the experiments I carried on this year. Although they ought to pay without difficulty the four or five half-pennies it costs to treat a bushel of bunt-infested wheat, since this bushel would sell at seven or eight half-pennies less than a like amount of the cleanest wheat and which it would bring on being treated; I dare not, however, believe that they would willingly incur this little expense and that they would make use of the solutions of soda, of potash, and pearl-ash lye. These materials are, in fact, worth four or five half-pennies per pound, each, and this amount of any one of them in the water bath is, as one may recall, what I used in moistening a bushel of seed for Part B of the plowed tract. I am not, therefore, insisting on the farmers' making special use of these compounds, which, in fact, are a bit dear and cannot be easily procured by everybody. Aside from the common aqueous solutions, the good effects of which we have noticed when preceded by the

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Everywhere there goes up a cry against this pest; everywhere the contagion is spreading; and nobody suspects the malign dust whence it [the pest] springs.

But, as the disease does not extend, by a great deal, over all the cantons, although it is generally present in every country, one can assume with some safety that there is only one-fourth of the Kingdom where the disease is consistently striking and one may estimate as a result of it a loss of ⅓ of the crop. On that basis it is conceivable that of 4 farmers, taken in the different Provinces of France, who have sown fields of equal fertility, and who have put in the same number of arpents, there is one whose wheat was infected with bunt and who has not harvested, solely because of this, more than 400 bushels of grain, while the other 3 obtained 600. That is to say, in summing up, these 4 farmers as a whole lost 1/12, or a little more than 8 per cent, of their total possible crop. There will, therefore, be available 2200 bushels of grain instead of 2400 that would have been had the wheat of the farmers not been stricken with bunt. M. Crollat observed that this disease had been prevalent for at least 50 years in the Canton whence he wrote. It has its noticeable beginnings, as is very obvious, like all pestilential diseases: It is noted down almost at the time it is observed; perhaps it was not previously recognized. A bushel of contaminated seed once introduced into Limagne established there, so to speak, the malady; and why would one not suspect that the disease, so considerable in France today, had its origin solely in a principle that was weak and absolutely foreign to the clime?

several baths in ordinary wash water, I urge the use of the putrefied urines: There is no danger of their injuring the seed; they cost nothing; and will have surely won the praise of those who have followed my advice. I shall soon be ready to tell them of the success I shall have experienced through the use of more or less concentrated greenwood-ash lye combined with lime. From the 8 combinations to which I have limited myself it will develop that the most expensive one will require an outlay of one *sol*, eight *deniers* per bushel and that the least expensive will be two *deniers*. Common ashes and lime can be had anywhere; the solutions can be made without difficulty; the treatment of the seed will be easy; and some experiments, the details of which have seen seen, seem to promise that this sort of remedy will produce the end sought.

FINIS

Original was from the Press of GUERIN & DELATOUR.

APPROVAL

I HAVE read, on the order of Monseigneur the Chancellor, a manuscript that has for its title: *Series of Experiments and Reflections on the Cause of destruction of the Grains of Wheat in the Head,* etc. and I have found nothing therein that should prevent its being printed. At Paris, the 5th of May, 1755. GRAVES.

AUTHORITY OF THE KING

LOUIS, BY THE GRACE OF GOD, KING OF FRANCE AND OF NAVARRE: To our beloved and faithful Councilors, the Officers holding our sessions of Parliament, Masters of the common Petitions of our Palace, Grand Council, Marshal of Paris, Bailiffs, Seneschals, their Civil Lieutenants, and to others of our Justiciary to whom it will appertain:

GREETING. Our much esteemed Sr. . . . We have shown that it would be desirable to print and to give to the Public a Work entitled Series of reflections relative to the Dissertation on the cause of the destruction of the grains of Wheat, etc. by M. Tillet, Director of the Mint of Troyes; if it please you to accord to him your Letters of Authority necessary thereto: TO THESE CAUSES, wishing to treat the writer favorably, we have permitted him and do permit by these Presents, to cause to be printed the said Work as many times as may to him seem desirable, and to sell it, cause it to be sold, and distribute it throughout our Kingdom for six consecutive years from the date of these Presents. We prohibit all Publishers, Booksellers, and other persons of whatever kind or condition they may be from introducing by a foreign edition into any part of our dominion as well also as to reprint the said work and sell it, cause it to be sold, distribute it or counterfeit it under whatsoever pretext without the express permission and by written decree of the author or of those who may act in his right, on pain of confiscation of the counterfeit copies and payment of a penalty of £3000 imposed upon each offender one-third of which will revert to us, one-third to the principal hospital of the town, and the other third to the author or to others in his right, and payment of all expenses, damages, and interest. On condition that these Presents will be registered at full length upon the Register of the Community (corporate body) of Printers and Booksellers of Paris within three months from this date; that the publication of said work shall be done in our Kingdom and in no other, on good paper and with excellent characters, in accordance with the printed page, attached for a model under the counter seal of the Presents, and that the Candidate conform to all Laws of the Bookseller and especially to that of the 10th of April, 1725; and that, before offering for sale the manuscript which will have served as copy for said Work, it be returned in the same condition to the office where Authorization will have been granted, into the hands of our very

esteemed and faithful Chevalier, Chancellor of France le Sieur DE LAMOIGNON; and that there will next be delivered two copies of said manuscript to our Public Library, one to that of our Chateau du Louvre, one to that of our very esteemed and faithful Chevalier, Chancellor of France, le Sieur DE LAMOIGNON, and one to that of our very esteemed and faithful Chevalier, Keeper of the Seals of France, le Sieur DE MACHAULT, Commander of our Orders; the whole on pain of the nullification of these presents; of the contents of which we order and enjoin you to do to please the Candidate and his assigns having cause fully and peaceably, without allowing it to cause them any trouble or hindrance. We desire that the copy of said Presents, which will be printed at length from beginning to end of said work, be held for one duly declared and that to the copies collated by one of our esteemed and faithful Counsellors and Secretaries proof be added as to the original. We first command our Bailiff or Sergeant on this order to put into effect the execution of all those required and necessary acts without requesting other permission and in spite of Clameur de Haro [the hue and cry], Charter of Normandy, and letters to the contrary. For such is our pleasure. Committed at Versailles, the twenty-second day of the month of April, the year of Grace one thousand seven hundred and fifty-five, and of our Reign the fortieth. By the King in his Council.

PERRIN.

Recorded on Register thirteen of the Royal and Syndical Chamber of Printers and Booksellers of Paris, no. 521, folio 406, in conformity with the Laws of 1723, which prohibit, Art. IV, all persons of whatever character and condition other than printers and booksellers from selling, retailing, or advertising any books for sale in their name, whether permitted by the authors or otherwise; on condition of furnishing the models prescribed by Article CVIII of the same law.

At Paris, the 9th of May, 1755.

DIDOT, *Syndic.*

MÉMOIRE

SUR LA CAUSE IMMÉDIATE

DE LA CARIE OU CHARBON DES BLÉS,

ET DE PLUSIEURS AUTRES MALADIES DES PLANTES,

ET SUR LES PRÉSERVATIFS DE LA CARIE.

PAR M. BÉNÉDICT PRÉVOST,

Membre de la Société de Physique et d'Hist. Nat. de Genève, de celle des Naturalistes de la même ville, et de la Société des Sciences et des Arts du département du Lot, séante à Montauban;
Correspondant de la Société Galvanique et d'Électricité de Paris, des Sociétés Médicale et de Méd. Prat. de Montpellier, de celle des Amateurs des Sciences de Lille, et d'Émulation de Lausanne.

C'est une botanique à faire, que celle des plantes microscopiques.
SENEBIER, *PHYSIOL. VÉG.* T. II. p. 291.

A PARIS,

CHEZ BERNARD, IMPRIMEUR-LIBRAIRE. QUAI DES AUGUSTINS, N.o 25.

1807.

Phytopathological Classics

NUMBER 6

MEMOIR ON THE IMMEDIATE CAUSE OF BUNT OR SMUT OF WHEAT, AND OF SEVERAL OTHER DISEASES OF PLANTS, AND ON PREVENTIVES OF BUNT

By

BÉNÉDICT PRÉVOST

Member of Société de Physique et d'Histoire Naturelle de Genève, Société des Naturalistes de Genève, and Société des Sciences et des Arts du département du Lot, séante à Montauban;

Correspondent of Société Galvanique et d'Électricité de Paris, Sociétés Médicale et de Médicine Pratique de Montpellier, Société des Amateurs des Sciences de Lille, and Société d'Émulation de Lausanne

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C'est une botanique à faire, que celle des plantes microscopiques
Senebier, Physiol. Vég. T. II. p. 291

Translated from the French

by

GEORGE WANNAMAKER KEITT

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FOREWORD AND ACKNOWLEDGMENTS

IN TRANSLATING a work of so much historical significance as characterizes the Memoir that follows, it has seemed more desirable to seek to retain as much as feasible of the mode of statement of the author than to convey his essential ideas in the style of another. The original text has, therefore, been followed as closely as seemed consistent with rendition in acceptable English. While an attempt has been made to eliminate the more glaring Gallicisms, constructions more characteristic of French than English have been retained in many instances in order to avoid the necessity of extensive recasting of the language. It is hoped that gain in fidelity to the original statement will more than compensate for the sacrifices sometimes imposed on the quality of the English.

Grateful acknowledgements are made:

To each, Mrs. T. C. Ryker and Dr. J. Dufrénoy, for comparing the translation with the original and offering many helpful suggestions for improvement of the English rendition;

To each, Dr. L. R. Jones, Dr. J. Dufrénoy, and Dr. H. B. Humphrey, for criticism of the sketch of the life of Bénédict Prévost and of the "evaluation" of the Memoir;

To the Lloyd Library for loan of the original copy of the Memoir and to the Princeton University Library for loan of the biographical sketch of Bénédict Prévost by Pierre Prévost (see footnote 1, p. 7);

And especially to Dr. H. B. Humphrey for comparing the translation with the original and critically editing it, the biographical sketch, and the "evaluation."

THE TRANSLATOR.

ISAAC-BÉNÉDICT PRÉVOST[1]
1755–1819

BÉNÉDICT PRÉVOST came of an old, intellectual family of Geneva, Switzerland. His parents, Jean-Jacques and Marie-Elizabeth Henri Prévost, were people of admirable qualities but limited means. They had two children, Isaac-Bénédict, born at Geneva, August 7, 1755, and a daughter, who died, unmarried, in early womanhood.

Bénédict's early education was very irregular. He had little inclination for the studies of grammar school, and was placed in a boarding school of a small neighboring village, where he received only very limited instruction. He undertook successively two apprenticeships, one in engraving and the other in commerce. He gave up the latter, which offered flattering prospects, to cultivate the sciences. In October, 1777, at the age of 22, he accepted the position of tutor of the sons of M. Delmas of Montauban, Département du Lot, France. This city came to be his second home, to which he was held for the rest of his life by ties

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[1] Prévost, Pierre. Notice de la Vie et des Écrits d'Isaac-Bénédict Prévost, 110 pp. Genève, 1820.

The following brief account of the life and works of Bénédict Prévost is drawn from this book by his cousin and life-long intimate friend and correspondent. Pierre Prévost's account is written with such authenticity, charm, and sympathetic understanding that it is abridged with as little change as possible, many passages being used in translation.

Abraham Prévost of Geneva, teacher ("principal du collège") and pastor, had two sons. The older became a lawyer of distinction. Pierre, the younger, born in 1751, became a distinguished scholar in the fields of literature, philosophy, and the natural sciences. In 1780 he was appointed by Frederick II of Prussia to the Academy of Sciences at Berlin, where he also held a professorship in an academy for young gentlemen. Though he was highly successful in this work, his love and loyalty for his family and country were such that, in 1784, he accepted the chair of Belles-Lettres at the Academy of Geneva, though the stipend was hardly a third of that at Berlin. Here he completed his long and distinguished career. (Cherbuliez, A. Discours sur la Vie et les Traveaux de feu Pierre Prévost, ancien Professeur de Philosophie à l'Académie de Genève, 64 pp. Genève, 1839.)

of friendship and by congenial opportunities for study and teaching.

At the time of his arrival at Montauban, Prévost had acquired little science; but he was very apt in learning it, and had especial talent and taste for mathematics. His progress was rapid. After two years he formed and announced the intention of devoting himself entirely to his studies. Though the very attractive positions of his cousin, Pierre Prévost, at Berlin were open to him when the former resigned to return to Geneva in 1784 (see footnote 1, p. 7), he preferred the friendships and the work that had become so dear to him at Montauban. He then persuaded his friends to refrain from seeking further offers for him.

The earlier phase of his studious career was concerned chiefly with mathematics. Physics and natural history dominated later.

In the study of natural history he had many obstacles to overcome, but also enjoyed some peculiar advantages. He spent part of the year in the country, the principal theater of his observations. There he found, in his adopted family, friends who shared his tastes and took pleasure in furthering his work. But the number was limited, and even in the city he did not find all the help that might have favored his enterprises. He had difficulty in procuring the books that he needed. The irregularities of his early studies served, perhaps, to arouse his curiosity; but they slowed his progress. He supplied by his genius and enthusiasm what he lacked in this regard, and his generous efforts were crowned by success.

His relations and literary correspondence multiplied. His correspondence with his cousin, Pierre Prévost, was never interrupted, and became for both a source of gentle and useful pleasure. At Montauban he was affiliated with *l'Académie du Lot,* of which he was one of the founders. He enjoyed the friendship of many members of this learned society. He had continuing relations, among others, with the able astronomer, le Duc la Chappelle. His pupils became his friends. At Geneva, MM. LeSage, Senebier, Jurine, Gosse, Huber, and Maunoir corresponded with him. Other learned societies that honored him with membership appear on the title pages of this translation and of the account of his life by Pierre Prévost (Loc. cit.).

Besides the memoirs that he read before the Academy at

Montauban, he published others through various scientific channels. Thus, by assiduous work, he laid the foundation of a solid reputation, gaining enjoyment from his discoveries and the continuous increase of his knowledge.

For him, the ardor for study did not at all interfere with the charms of friendship. The gentleness of his dealings, his amiable gaiety, the sureness of his character and his principles, endeared him to all with whom he had associations. The very manner in which he had acquired his great knowledge contributed to its value. He was self-taught. Everything in his writings and his discourse was marked with the stamp of originality. He had invented what he learned; and, although long accustomed to draw from books instruction that he well knew how to appreciate, he always did this work in an active manner, meditating, repeating observations, varying experiments, judging his masters freely, and recognizing in the last resort nothing but his reason and nature.

Content to follow his tastes and the development of his interests in a quiet activity, Bénédict Prévost thought little of the avenue. But his friends thought for him. A protestant academy (*Faculté de Théologie protestante*) was founded at Montauban. There was needed a professor capable of filling with dignity the chair of philosophy. All eyes turned to Bénédict Prévost. The position was offered him, and after careful consideration he accepted it (in 1810). Thenceforth, he directed all his efforts towards the instruction with which he had been charged. He endeared himself to his students and was truly devoted to them. His courses were followed eagerly and fruitfully.

On June 10, 1819, at the age of 64, after a short illness, Bénédict Prévost died at Montauban in the home of M. Delmas, universally regretted.

The works of this remarkable man are listed and discussed by Pierre Prévost (Loc. cit.). More than a score of papers were published in the following channels: *Annales de Chimie, Annales de Chimie et de Physique, Bibliothèque britannique, Bibliothèque universelle, Journal de Physique,* and *Annales de la Société pratique de Montpellier*. His unpublished works were still more extensive. The subjects that he treated covered a broad scope, including physics, chemistry, biology, and philosophy.

Bénédict Prévost's most important contribution is the Memoir here translated. It is sad to record that this beautiful work, the crowning achievement of a life so singularly devoted to the advancement of knowledge, was denied fitting appreciation[2] during the life of its author. Following the presentation of this paper at Montauban in 1807, the Society published it and sent it to the Institute, which appointed a Commission to examine it and make a report. M. Tessier, reporter for this Commission, "lauded the zeal, the acuteness, even the talent, of M. Bénédict Prévost in microscopic observations, but he did not at all recognize the strides of genius that he had taken along a course in which men of the highest merit had hitherto but groped."[3] This cold indifference, suffered from a body so well informed and justly celebrated, would have profoundly depressed a less modest man than Bénédict Prévost; but, far from protesting against this denial of justice, he made a law of silence, and imposed it, even to his last moments, upon his many friends. However, the preventive that he recommended constantly guaranteed the crops of proprietors who used it with care. His Memoir[4] penetrated into Belgium, England, and other countries, and foreigners profited by a discovery made in France, before the prejudices in favor of the old methods gave way. So it was that vaccination found apologists long after the discovery of vaccine.

A further discussion of the significance of the Memoir is given in the following "evaluation" by the translator.

G. W. Keitt.

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[2] The following statements are drawn from an article quoted by Pierre Prévost (Loc. cit.) from *Journal du Tarn et Garonne*, July 21, 1819.

[3] All quotations in this sketch and the appended evaluation of the Memoir are given in translation.

[4] Pierre Prévost (Loc. cit.) states that a very good abstract of the Memoir appeared in *Recueil agronomique de la Société des Sciences de Tarn et Garonne*, September, 1820.

AN EVALUATION OF THE MEMOIR BY THE TRANSLATOR

IN THE FOLLOWING Memoir, Bénédict Prévost clearly showed "that the immediate cause of bunt is a plant of the genus of the uredos or of a very nearly related genus." So far as the translator is aware, this work contains the first recorded adequate experimental demonstration and interpretation of the rôle of a microörganism in the causation of a disease. It establishes one of the most fundamental and fruitful scientific concepts and lays a firm foundation for the later and more generalized germ theory of disease. The Memoir contains many other contributions of outstanding merit, but this alone entitles it to recognition as one of the most significant pioneer works in the field of biology.

Prévost's contribution bears critical examination. Founded upon able, painstaking experimentation over a ten-year period in the prime of his life, his essential findings stand unchanged. He gave a detailed and accurate description of the symptoms of bunt in the various macroscopic stages of its development (¶1–8, 29–33). He noted the constant occurrence of "globules" in the bunted kernels and rightly suspected and proceeded to prove that they were the "seeds," "gemmae" or "spores" of a cryptogam (¶2, 5, 10–13). He described and illustrated these spores in detail (¶5–7, 24–27, 52; Pl. 1) and made extensive studies of their germination and the development of the "bunt plant" in relation to time, temperature, substrata, toxic agents, age and previous treatment of the spores, concentration of the spores, etc. (¶11–28, 41–43, 50, 52–56, 109–113, 121–128, 130, 138–158, 188; Pl. 1). Having concluded that he was dealing with a microscopic plant "of the genus of the uredos or a very nearly related genus" (¶102), he proceeded by extensive and successful inoculation experiments to prove that it is the "immediate cause" of bunt and to define conditions that favor or hinder infection (¶57, 104–128, 137). He pointed out "that the vegetation of this

plant, as well as that of the majority of the uredos, begins in the open air and is completed in the interior of the plant that it attacks" (¶102), suggesting for such organisms "the general denomination of *internal parasitic plants*" (¶103). He observed germinated spores of the bunt organism in the soil (¶107, footnote) and on the surface of wheat seedlings grown in infested soil (¶43). Though he did not succeed in observing the mode of penetration of the bunt fungus into the wheat plant or its growth to the wheat embryo, he correctly surmised (¶46) that some ramifications of the bunt plant must penetrate into the very young wheat plant and later insinuate themselves into the embryo and fructify (¶41–58). He observed the bunt plant after it reached the wheat embryo and described its fructification there (¶33–40, 91, 93). He was unable to trace the details of the earlier stages in the formation of the gemmae and had an erroneous idea regarding the development into gemmae of some of the smaller granules that he took to be their initials. However, he correctly recognized the more advanced young gemmae (Pl. 2) and found that they germinated typically (¶36). In extensive and refined toxicological studies (¶126–130, 138–156) he found that certain copper salts, distilled water in which metallic copper had been left under suitable conditions, and various other substances in solution would prevent the germination of spores of the bunt fungus. He critically distinguished injurious and inhibitory from lethal effects and experimented extensively on relations of concentration of the toxic agent, time, and temperature to toxic effects. On the basis of the information thus obtained and of his extensive knowledge of the disease, he proceeded to experiment on the control of bunt. He made suitably controlled large-scale field tests in which inoculated seed wheat was planted after having received various treatments. Spores of the bunt organism from the treated seed were tested for germination, and data were taken on the development of the disease on the wheat grown in the field from the experimental seed. Excellent control of the disease was obtained by steeping the seed wheat in a copper sulphate solution, and detailed practical recommendations were made for performing this operation on a large scale (¶157–181).

In basic method and in interpretation, Prévost's work is sur-

prisingly modern in the best sense of the word. Throughout his investigation he consistently used the best approach that has yet been found to the secrets of nature, the experimental method skillfully applied from the point of view of history of development. His work is remarkably comprehensive and well correlated and lays the foundation for nearly all major branches of modern plant pathology.

Prévost's more detailed techniques were in keeping with the quality and principles of his general plan. The methods that he developed for growing the bunt fungus in water culture, for example, were far ahead of his time. He controlled the quality of the water with reference to source, purity, and sterility; the quality of spores with regard to source, age, and previous treatment; and the conditions of the experiments with regard to such factors as temperature, moisture, availability of oxygen, quantity of spores and of water, and introduction of a wide variety of chemical compounds for study of their toxicity in terms of injurious, inhibitory, or lethal effects (¶41–43, 109–113, 121–128, 130, 138–158, 188). He developed effective methods for minimizing contamination in the spore suspensions that he prepared for these experiments, using in part the principle of dilution. He clearly recognized the occurrence of contamination of cultures by air-borne spores or microörganisms and devised ingenious methods for its prevention (¶41). Here was laid an excellent foundation for the pure culture of microörganisms *in vitro* (¶103).

An example of Prévost's technical thoroughness and penetrating insight is found in his discussion of effects of "hard" water on the fungicidal action of dilute solutions of copper sulphate (¶152–153). He calls attention to the formation of a precipitate at the expense of the copper sulphate "in greater proportion as its [the copper sulphate's] quantity in relation to that of the water is reduced; so that, *if a very weak solution is filtered, the liquid that passes through does not produce nearly so great an effect*. . . . Does the precipitate act upon the bunt; or, indeed, *could the filter act upon the sulphate?* However it be, the precipitate and the solution in which it is suspended act, together or separately, upon all the bunt that the solution is capable of completely wetting." [The italics are added.] While he did

not solve the problem of whether the filter acts upon copper sulphate, he clearly recognized and defined it. It is now easily demonstrated that the filter may retain most of the copper of a small quantity of dilute copper sulphate solution by adsorption. Many workers who followed Prévost in studies of the toxicity of copper fungicides failed to take this vitally important question into account. Much of the resulting confusion could have been avoided if his pioneer work had received the attention that it deserved. Furthermore, suitable studies of the fungicidal properties of Prévost's precipitate, which is closely related to the essential components of Bordeaux mixtures and their weathered residues, might well have hastened by many decades the discovery of fungicides of this type.

Prévost's interpretations are critical and discriminating and worthy of the quality of his experimentation. His conclusions are rigorously based on experimental proofs. In treating some questions concerning which the experimental evidence was incomplete, chiefly in relation to diseases that he necessarily studied less intensively than bunt (¶59–102), he justifiably uses analogy, some of which proved to be erroneous. However, when he resorts to analogy he does it frankly and deceives neither his reader nor himself regarding the basis of the reasoning (e.g., ¶ 102). His work on other diseases (¶59–102) was incidental, and the quality of much of it is far below that of his essential contribution, the study on bunt.

His conception of parasitic disease is remarkably comprehensive and discriminating. He points out that the internal parasitic bunt plant is the immediate cause of the disease but makes it equally clear that the parasite can induce the disease only under sufficiently favorable conditions (¶104–128 and 137). In his title he refers to the bunt plant as the "immediate cause" of the disease, and in the text he usually speaks of it as the "immediate" or "direct" cause. However, having clearly established the qualifications that attach to the causal rôle of the organism in relation to the disease, he does not hesitate in later passages to speak of the bunt plant as the "cause" of the disease. Prevost's usage deserves the careful consideration of those who are interested in clarity of concept and preciseness of terminology regarding these relations, which are so fundamental to general pathology.

A perusal of Prévost's Memoir will reward one with many interesting by-products of his pioneering. His description of indirect germination of the "uredo with white dust" from *Portulaca* (¶95) is probably, as he thought it to be, the first record of this type of germination. In ¶96 is found a vivid and accurate description of what later became known, under a more generalized concept, as Brownian movement. Again, though his mention of the matter is incidental, his increasing knowledge of the development of microscopic plants and "animalcules" (¶95) from pre-existing "gemmae," "spores," or "seeds" leads him to express his disbelief in ideas of spontaneous generation that were then prevalent (¶95 *h*).

Prévost's ideas on the control of bunt are as clear and advanced as his conception of its nature, cause, and epidemiology. The ideal expressed in his closing sentence, . . . "to propagate pure seed, and to extirpate the germ of the infection from the soil of the Empire," should be a lasting inspiration to students of plant disease control. Even with all the splendid achievements of modern plant pathology, how far short we are of this ideal! Of course, there are many obstacles that Prévost could not foresee. But would he have been content to stop, even in the face of great difficulties, with methods that are in so many cases costly, laborious, and uncertain of success and that so often consist of an annually recurring protective program, with little or no attention to "extirpation" of the pathogen? May not some of the brightest chapters in the history of plant-disease control remain to be written on the basis of knowledge gained by pursuit of the ideals and application of the methods of this far-visioned pioneer? May not sufficient application of the experimental method to the study of the phenomena of plant disease from the point of view of detailed history of development reveal new possibilities for supplementing protective programs with such effectiveness that many destructive plant pathogens, even if they are not "extirpated from the soil of the Empire," can be reduced to such low levels of survival that they may be surely and economically controlled, or even eradicated locally or regionally? Surely one should give up such ideals "only when repeated and unsuccessful efforts force him to renounce all hope" (¶9).

In failing to gain due recognition from his generation, Prévost

but shared the common lot of pioneers in the advancement of knowledge. He was too far ahead of his time. The autogenetic theory of disease was dominant in places of authority. Who was this obscure tutor from the provinces that he should challenge the teachings of the Masters?

While it was not given the recognition that it deserved, Prévost's contribution was never entirely overlooked, and undoubtedly had an important influence on later work. It has already been stated (p. 10) that the Memoir was well reviewed in at least two popular journals and that the method of seed treatment became widely known through practice. The Memoir also was cited in botanical literature. Of special importance is the extensive reference made to it by the Tulasnes,[1] with every evidence of confidence in the work and acceptance of its essential conclusions. Such treatment in a major publication by the leading mycologists of the time brought Prévost's work into conspicuous notice. Thereafter, he was widely cited, but chiefly for the germination of fungus spores and the discovery of the fungicidal action of copper sulphate. The full significance of his contribution continued to lack recognition, probably due largely to reliance of later workers upon reviews, the original being very rare.[2] DeBary,[3] for example, in the classical work that is correctly credited with establishing beyond further serious opposition the view that fungi are capable of causing disease in plants, cites reviews and states that the original work was not available.

In contemplating the quality and significance of the work, one's thoughts revert to the man. Then the characterization of the biographer (Pierre Prévost, loc. cit., p. 19–20), already notable for its intimate and sympathetic understanding, reveals a new depth of insight:

[1] Tulasne, L. R. et C. Mémoire sur les Ustilaginées comparées aux Uredinées. Ann. Sci. Nat., Ser. 3, 7: 12–127. 1847.

[2] To gain information regarding the availability in North America of Prévost's Memoir and of the account of his life by Pierre Prévost (Loc. cit., footnote 1, p. 7) an inquiry was sent to more than 50 libraries that were thought to be the most likely to contain such works. Only one copy of each was located (see acknowledgments, p. 5). Grateful acknowledgment is made to Mr. C. S. Hean, Librarian of the Wisconsin College of Agriculture, for assisting in this search.

[3] DeBary, A. Untersuchungen über die Brandpilze und die durch sie verursachten Krankheiten der Pflanzen mit Rücksicht auf das Getreide und andere Nutzpflanzen, 144 pp. Berlin, 1853.

"For him, the ardor for study did not at all interfere with the charms of friendship. The gentleness of his dealings, his amiable gaiety, the sureness of his character and his principles, endeared him to all with whom he had associations. The very manner in which he had acquired his great knowledge contributed to its value. He was self-taught. Everything in his writings and his discourse was marked with the stamp of originality. He had invented what he learned; and, although long accustomed to draw from books instruction that he well knew how to appreciate, he always did this work in an active manner, meditating, repeating observations, varying experiments, judging his masters freely, *and recognizing in the last resort nothing but his reason and nature.*" [The italics are added.]

Those who cherish the memory of men who have pioneered greatly in the advancement of knowledge and the amelioration of suffering may well pay a full measure of respect and admiration to this modest scholar of Montauban, who made such splendid "strides of genius along a course in which men of the highest merit had hitherto but groped."

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MEMOIR ON THE IMMEDIATE CAUSE OF BUNT OR SMUT OF WHEAT, AND OF SEVERAL OTHER DISEASES OF PLANTS, AND ON PREVENTIVES OF BUNT

THE RAVAGES of bunt are known. It often devastates the country in which I live. I have seen fields in which there were twice as many bunted as healthy heads; and it is not very unusual to find the latter in the proportion of only two or three to one.

MM. Tillet and Duhamel, and later several other authors, especially M. Tessier, have contributed excellent works on this scourge of agriculture, and have indicated various means of protecting the fields from it. But, whether these means were too troublesome or too costly, or whether they did not always measure up to the expectations of those who put them into practice, it is a fact that many proprietors often have their crops infected, although all or nearly all have adopted some procedure of liming, or other preventive methods.

It is this that, in 1797, induced the scientific section of the Society of Montauban, after the reading of a memoir by M. Robert-Fonfrède upon this subject, to invite its members to occupy themselves with it.

Thenceforth, I prepared to respond to this invitation. Every year, on the estate of M. Delmas, near Montauban, I made large-scale experiments of which I shall speak in treating of the preventives of bunt. The first question concerns the immediate cause of this deterioration of the grain, which I discovered in 1804, and of which I have assured myself since by a large number of experiments and observations.

But I shall first give an exact idea of what is called bunt, whether from my own observations or from those of authors who have shed the most light upon this matter.

I

Description of Bunt of Wheat

1. *Bunt*, which in some countries is called *smut*, attacks the interior of the kernels, without changing the nature of the glumes, the petals, or the other parts of the head, which, however, as well as the stalk, it deforms more or less.

2. The germ is destroyed, and the farinaceous substance is replaced by a brown, almost black, powder, which has a very bad odor, especially when it is fresh.

3. The mature heads that bear the bunted kernels do not incline on their stalks, as do the healthy ones; but, because of their light weight, the direction of their axis remains vertical. They are distorted, and the upper part of the stalk, immediately beneath the head, is more or less twisted. In general, the bunted plants are shorter. The heads and leaves have a blackish tint; and it is very noticeable that, not only the straw of the bunted wheat, but that of the healthy wheat mixed with it, is darker than the other.

4. The bunted heads do not flower; the stamens are formed, however, but they are compressed between the glume and the seed, and do not emerge at all, or at least appear only when the wheat is fully ripe, and when by desiccation or some other cause the envelopes fall. The stigmas are present also, but they do not expand as in healthy wheat, or at least expand only very rarely.

5. When the dry powder of bunt is examined under the microscope with a very strong lens, it is seen to be composed of brown, nearly black (Pl. 1, fig. 1), rather uniform, and roughly spherical grains, in which some more or less luminous points are perceived by transmitted light, as if they themselves contained other much smaller grains.

6. As exactly as I could, I measured the diameter of the first grains by means of a glass on which had been drawn the 1/200 parts of a line. The smallest ones appeared to me to equal one of these measures, or 11/1,000,000 meter; and the largest 1/150 line, or 15/1,000,000 meter. A bunted kernel of medium size should, therefore, contain several millions of these grains.

Held for some time in water, they swell a little, and acquire

a more perfect sphericity (Pl. 1, fig. 2). The largest then measure as much as 1/100 line, or 23/1,000,000 meter.

7. These grains or globules for the most part sink rather quickly to the bottom of the water; but, though they are consequently specifically heavier than the liquid, some of them nevertheless stay at the surface, and a bunted kernel of wheat rather generally remains there. Then it often empties itself through some rent, from which the black dust that it contains escapes like a descending smoke.

8. One sometimes finds on the same wheat plant bunted heads and healthy ones; in the same head, healthy and bunted kernels; and the same kernel may be only partly bunted. This last case is rare; however, I succeeded in collecting two or three hundred of these kernels, the majority of which appeared intact on the germ side. I wished to sow them; but, having accidentally lost them, I made up for the deficiency of this experiment in a way that will be seen later.

II

Physiologic Cause of Bunt

9. In making experiments relative to the economic aspect of the question that I had undertaken to solve, I did not fail to apply myself to the direct cause of bunt. I had found in the authors whom I had been able to consult only conjectures upon this interesting point; but no observation, no decisive experiment, or undertaking in the hope of obtaining decisive results.

However, the immediate cause of the trouble once known, it would seem to be very much easier to find a remedy. One should, therefore, give up finding this cause only when repeated and unsuccessful efforts force him to renounce all hope.

10. The form and specific gravity of the globules of bunt, which I had often examined under the microscope, the nature of which I had tried in various ways to change, which I could not help recognizing as organized bodies, and which I likened to globules of the rusts (*uredo*, de Cand.) and several other microscopic plants, made me suspect that they were the seeds, or, to speak more exactly, the *spores* of some cryptogam.

11. I had seen the globules of certain *uredos* that I kept in

water upon some leaves, to try to discover the manner in which they reproduce, put forth long stalks, which I shall describe elsewhere. On the 23rd of the Prairial of the year 12 (June, 1804) I attempted to suspend, in the bottom of a tumbler, the powder from three or four bunted kernels with a little water, which I added up to the height of one or two centimeters (one inch). I covered the glass, and left it in a quiet place. From time to time, with lenses of different strength, I examined a few drops of this water, in which there was always a rather large number of globules.

12. After three days, the decimal thermometer being held at a mean of 18° (14.2° R.), I found that several globules had put forth stalks, a small number of which bore an aigrette (Pl. 1, figs. 17–26) formed, apparently, of long, narrow leaves. Others showed only small buds (Pl. 1, fig. 3), which had just emarginated the integuments. Lastly, the majority still showed no change. But, continuing to observe in the same manner, one found each day a greater number of stalks, either simple (Pl. 1, fig. 4–9) or terminated by aigrettes, or by sorts of tufts, that is to say, by aigrettes the shoots of which were brought together and pressed, a little twisted and as if connected at the tip (Pl.1, fig. 27). The latter afterwards bear small oblong bodies that seem to be their fruits (Pl. 1, figs. 28–32). I have very often repeated this experiment, and it always has succeeded.

13. The grains or globules that make up the bunt dust are, therefore, the seeds (gemmae, gemmules, or spores) of a microscopic plant, and this same plant is the cause of the disease of the kernels.[1] But in order to place this truth in its full light I

[1] It had been thought that the bunted kernel was a kind of lycoperdon, an opinion that could not be sustained, and that, in the present state of our knowledge, its illustrious author probably would not offer. In the year 13 (1804), MM. Fourcroy and Vauquelin gave a chemical analysis of bunt, which had led them to regard this substance as a degeneration of the gluten of flour; but it was impossible that a chemical analysis alone, however perfect it might be, could lead to the discovery of the true cause of the disease. No special mention of bunt as a plant was made in the botanical part that follows the natural history of Buffon. M. de Candolle does not speak of it at all in the new edition of *Flore française*, although he cites the name as one of those given to *smut* or *blight*, which, as is known, is not the same disease. He, therefore, did not then regard bunt as a plant. It is true that later, towards the middle or the end of 1806, at about the time when I made known the detailed history of this cryptogam to the *Société de*

shall make known in detail this plant, its progress, and some conditions that influence the manner of its development.

III

Description of the Bunt Plant; Its Various Developments in Water and in Air

14. After the bunt[2] globules have remained from 60 to 72 hours in water or on some very moist substance that cannot maintain its growth, such as earth, paper, etc., the thermometer being held at a mean of 18° of the division in 100 parts (about 14.5° R.), one perceives upon some of them, on examining them under the microscope with a very strong lens, that the integument is slightly emarginated; then one notes, in transmitted light, a slightly elevated white point, which swells more and more (Pl. 1, fig. 3), and later lengthens, so that after five or six hours it takes the form of a small cylindrical stalk[3] (Pl. 1, figs. 4–7), the diameter of which sometimes reaches a half, sometimes only a third, and most often a quarter of that of the gemma.

15. I have said (7) that a part of these gemmae or globules stay at the surface of the water, and that the majority go to the bottom. I shall add that during growth some of the former sink, and that some of the others rise. Those that stay constantly at the bottom of the water, which are very numerous, often unite by a kind of gluten, exsert long, simple, slender stalks (Pl. 1, figs. 8 and 9), of which the part nearest the gemma is ordinarily so transparent that it is distinguished with difficulty, if the light be the least unfavorable. When it is like this, or when one observes it dry, this part of the stalk appears articulated, or, rather, composed of small cylindrical cells of its own diameter, and separated by transverse partitions more or less near together (Pl. 1, fig. 8). The other part is less transparent, and one is rarely able

Physique et d'Histoire naturelle de Genève, this learned scholar read at the Institute a memoir in which he says that bunt probably comes from the same origin as the rust and smut of wheat, the germs of which he thinks gain entrance through the roots with the nutrient juices of the plants.

[2] This should apply to bunt of the current year: the older the bunt, the more time it takes to germinate.

[3] The little bunt plant has no roots; its seeds, therefore, have no radicle, in which respect, among other things, it differs from the best known seeds.

to perceive cells in it. It does not grow as much as the former. It apparently is a sort of sheath or case, which originally envelops the whole stalk, and which the latter, by its extension, forces to become detached from the gemma.

16. The stalks that grow at the surface of the water, or that rise to the surface, are ordinarily larger. Scarcely have some of these acquired the length of one diameter of the gemma, when their tip appears black.[4] Soon this tip becomes granular (Pl. 1, figs. 12 and 13), and takes the form of a small star (Pl. 1, figs. 14 and 15), the rays of which, at first very short, later elongate like narrow leaves somewhat similar to those of certain liliaceae; so that the entire plant is not remotely unlike a young onion plant with its leaves and its bulb, especially when, after having begun its growth in the air, it sinks with its aigrette and finishes growing in the water (Pl. 1, figs. 22–25); or else it is extended in a stupose mass that seems to be composed of slender filaments, pressed against one another, terminated in a sharp end, a little distended in the middle, as if twisted and fastened near the tip (Pl. 1, fig. 27).

17. Let us distinguish, for greater clarity, the simple stalks (15), the liliaceous, and the tuft-like or stupose stalks (16). Let us distinguish in the first the sheath that bounds them, from the transparent part that is attached directly to the gemma; and in the others, the stalk, properly called, and the *leaves*, although, as will be seen later, I am far from regarding the shoots of the aigrettes as true leaves.

18. The leaves often arise quite near to the gemma, upon a very short stalk. Sometimes, on the contrary, this stalk is rather long, and one may then ordinarily perceive the elements or cells of which it is composed.

19. The young leaves are nearly always opaque, and consequently appear dark to transparent, especially when growth is vigorous.[5] In this case the liliaceous leaves are distorted, obtuse, and of a certain firmness. When the growth is weak, or when the

[4] *Black*, that is to say, *opaque*. In reflected light this part is white, as well as the little tuft of leaves that will be discussed, and that develops from it (Pl. 1, fig. 26).

[5] When the light reflected by the mirror under the object carrier is very strong, the leaves, which have grown out of the water, and are opaque only because of their tenuity, refract it like a prism, and appear to be colored.

star develops in the water after being formed at the surface, the leaves appear larger, more pointed, more flexible, and more transparent. They form, at first, a little pad upon the stalk. The simple shoots that grow at the bottom of the water (16) are ordinarily very flexible and elastic, usually a little undulated or tortuous. Sometimes they appear forked or provided with antler-like processes, but then they are thick and short (Pl. 1, figs. 10 and 11). Or else, without changing form and without leaving the water, they may acquire a length of as much as 45 or 50 times the diameter of the average gemma.

20. The three kinds of stalks seem disposed to assume a vertical direction; but they are often recumbent, because they fall down.

21. One, two, or three days after growth has begun, there are observed upon the leaves of the tuft-like stalks small, oblong, subcylindrical bodies, slightly recurved, lightly compressed, the length of which, double, triple, or quadruple the width, attains as much as twice the diameter of an ordinary gemma (Pl. 1, figs. 28–32). These small bodies are borne on a pedicel, often very short, sometimes rather long, and a little twisted. I have observed on some of them slight constrictions that seem to indicate the segments or partitions (Pl. 1, fig. 32). Sometimes, also, I have thought I saw them filled with little granules or extremely small globules; but these cases are very rare.

22. The majority of the liliaceous leaves fall after some time. Those of the stupose stalks expand (Pl. 1, figs. 33 and 34), and also sometimes fall. All become transparent; they are then very narrow; so that it appears that those still clinging to the stalk show a certain width or appear obtuse, and are composed of several leaves combined.

23. The expanded parts of the stupose stalks often acquire a considerable length, become serpentine or interlaced in a thousand ways, and sometimes become branched (Pl. 1, figs. 37 and 38). The fruit-like growths that occur on the same stupose stalk often remain attached when the latter develops, and change their relative position, like flowers or fruits on branches that continue to grow. All that is much better observed when, the gemma remaining in the water, the tip of the stalk or of the *leaves* is outside in very moist air. By degrees everything disappears, and

after a few months nothing remains but some granules or detached globules, sometimes quite distinct. I shall state below the precautions necessary to make this delicate observation clearly.

24. When the gemmae have remained in the water for one or two days, and when, consequently, they have become a little more transparent, the clearest area observed in transmitted light is towards the most elevated part of the hemisphere observable; which proves that the opacity comes from the integument rather than the interior of the globule.

25. The middle of the gemmae that have produced stalks of a certain length appears flattened or even concave, translucent, of a reddish-yellow tint, as if metallic. A sort of rose engine pattern [the reticulations] of a certain regularity is observed. These gemmae are evidently empty; and as the envelope peripherally presents to the eye a greater thickness in perspective, one might think them surrounded by an opaque pad (Pl. 1, figs. 32 and 36).

26. Sometimes, after a very prolonged stay in water, the exterior integument, which is of the rose engine pattern and colored, ruptures and becomes detached by fragments. It then leaves exposed an empty, colorless, transparent globule. The latter originally contained the embryo of the stalk, which was itself enveloped, as we have seen, by a sort of case or sheath: thus this embryo is at first under three integuments or envelopes.

27. Before germination, the gemma can be forced to discharge its contents by pressing it lightly in a drop of water between two flat glasses. It presents then precisely the same appearance as when it has exserted a long stalk. There emerge from it several very irregular globules, or shapeless masses, some of which are so small as to be seen with difficulty under the strongest lenses. These globules or molecules, therefore, constitute in the gemma the embryo of the stalk. This embryo is difficult to see in bunt; but I shall give the history of the embryo of a plant of the same family, whose embryo, which is perfectly made out in the gemma, diminishes in proportion as the stalk grows, and finally disappears entirely when the latter has acquired a certain length.

28. The granules of §23 probably would become gemmae similar to those from which they originate, if they were suitably placed, that is to say, in the embryo of the seed of some young

head of wheat. Of this the following observations scarcely admit any doubt.

IV

Appearance and Progress of Bunt in the Embryo of the Kernel

29. Towards the middle of May, bunt can be recognized in the fields due to be infected by it; but it must not be sought in the heads then emerging: those that harbor it are, on the contrary, still enveloped by leaves, and are on the shortest stalks.

30. The bunted wheat plants also are those whose leaves are most laden with that yellow crust, commonly called *rust*, a species of *uredo* of de Candolle.

I do not maintain, however, that rust and bunt always occur together; I say only that, when the wheat is attacked by these two diseases at the same time, the bunted plants also are those whose leaves are the most heavily laden with rust.

31. This crust appears, at least very noticeably, only on the most fully developed leaves; and, consequently, the leaf whose base still envelops the head, and may be regarded as the *terminal one*, also is the last on which this parasitic growth appears.

32. Now, in general, the bunted heads are found under the base of a *terminal leaf* which is already more or less laden with rust, although there is little or none of it on the terminal leaves of other culms of the same height or whose heads have attained the same degree of maturity.

33. The embryo of the bunted head bears anthers of a more yellowish green than those of healthy, no more advanced heads; the lobes of these anthers are deformed, contorted, and turned downward at the bottom like an arrowhead; the stigmas are more or less disposed to expand; the pistil, which is larger, is of a dark green, or bears externally a patch of this color (Pl. 2, figs. 1–9); crushed between the fingers, it has a bad odor, and it is filled with a greenish gray or white paste mottled with green.

34. If one takes a little of this paste with the point of a needle, and places it on the object carrier in a drop of clear water, he observes a great number of globules of different sizes, from the smallest that can be seen with the strongest lenses to globules as large as the gemmae of mature bunt (Pl. 2, figs. 11–21).

35. The most advanced heads are those in which the largest globules are the most numerous, and reciprocally; so that one finds only the medium or small ones in the very young embryos; but they perfectly resemble those of the same size from the most advanced heads. I have seen such globules in heads that were only 22 to 23 millimeters (10 lines) in length, not to speak of those about which there can be any doubt.

36. These globules are certainly the bunt gemmae in a state of imperfect maturity; for, aside from the fact that their like is not found in healthy wheat,[6] the most advanced germinate in water, producing stalks like these gemmae do; only it takes them longer, all other things being equal; and the eye can travel from the largest of these globules to the smallest by absolutely imperceptible gradations.

37. Some, generally the largest, seem to have grown like small sessile fungi on shapeless fragments of a firm material (Pl. 2, figs. 19 and 20); others on a sort of delicate, transparent spawn, which dilates in water, like a sponge, and is encountered again in the water-suspended dust of the fully mature bunt. Finally, some smaller ones appear to be attached in clusters (Pl. 2, fig. 14).

38. As to the minute description of these globules, I shall say that they have the remarkable transparency and clearness of *pure water;* that in general, excepting the largest, they are rather spherical, save for the flatness due to the excess of their specific gravity over that of water; that some appear empty or filled with a homogeneous translucent substance; that others seem to contain one or several secondary globules of very variable sizes.

39. The largest of these secondary globules, real or apparent, are in the interior; but the smallest, assembled in piles, occupy the surface. They are portions of the outer integument, which is beginning to form (25). These differences of position become evident when gemmae, not yet very mature, are rolled over in a drop of water.

40. One notices in the embryo of the seed of healthy wheat and in this seed itself, as it approaches maturity, several other

[6] An exception must be made of the smallest, which cannot be distinguished from those found not only in the embryo of the seed of healthy wheat, but also in nearly all parts of other plants.

kinds of globules, which it is very easy to distinguish from those we have just described (the smallest excepted). One kind are flattened (Pl. 3, fig. 1), and refract the light, so that they sometimes appear to be blue or very light green. They are more uniform than those of immature bunt, and the volume of the largest does not equal that of an average gemma of the latter. Other globules, seen in very large numbers in wheat that is still in the milk, have, in transmitted light, the appearance of mother-of-pearl. They are very irregular, and larger in general than those of bunt. They are perhaps only drops of a liquid that is immiscible with that in which they float. Neither kind contains secondary globules. The globules of pollen sometimes accidentally become detached from the anthers long before they are mature, and become mixed with the others. They are large, nearly uniform, compressed, often heaped up like piles of coins. The interior of the embryo seems sometimes, by transmitted light, to be filled with globules, but this is ordinarily only an appearance, and these fictitious globules do not become detached.

Now that it is well proved that bunt is a plant, the spores of which germinate in the open air and can be detected long before their maturity in the wheat embryo, it remains for us to investigate how they gain entry. This is the purpose of the following section.

V

Studies on the Introduction of the Bunt Plant into That of Wheat. Experiments and Observations Relative to This Subject

41. One means of finding how the bunt plant enters the wheat plant is, it seems, to observe it with care throughout all the stages of development that it is capable of exhibiting.

I said in §23 that I would return to the observation there described, and that I would indicate the means of making it with clarity. The manner in which I procured the first bunt plants that I observed has been seen in §11. There are a few additional precautions to be taken when one proposes to follow them for a very long time.

a. Instead of suspending at one time the dust from two or three bunted kernels of wheat, one makes a suspension of that

from only one, and at the same time removes the shreds of epidermis or bran, from which the dust falls away, the stamens, etc., which are easily seen.

The glass is about three-fourths filled with distilled water, and allowed to settle for eight or ten minutes.

Most of this water is decanted, leaving only as much as is necessary to prevent losing the bunt that is at the bottom.

It is stirred, the glass is filled anew, and when the bunt has settled, it is emptied as the first time; it is filled again, is covered to protect it completely from dust, without, however, hermetically sealing it, and allowed to stand.

b. It is necessary to use very pure and recently distilled water, so that it may not have been long exposed to the open air, from which are always deposited dust, spores of moulds, or other microscopic plants, which grow there and interfere with the experiment. This precaution is further necessary to prevent the development of a large number of monads and other animalcules, which, in their movements, break the most delicate parts of the little plant that we are about to observe.

c. The gemmae of bunt begin to put forth stalks

$2\frac{1}{2}$ or 3	days after immersion at a mean temperature of	17 or 18° C.
3 4		14 15
5 6		11 12
9 10		7 8
11 12		4 5

d. At about the time when, according to the temperature, it is supposed that growth has begun, one takes from the surface of the water of the first glass a little of a sort of pellicule with which that surface appears to the naked eye to be lightly veiled, and puts it into a drop of very pure water on the object carrier.

e. At this stage, the stalks of the three forms (17) are ordinarily little advanced, and one still sees a few gemmae that seem to have undergone no change. Within a day or two, several tests are made that may be observed without other precautions. But when the growth is more advanced, and when the fruit-like growths (12 and 21) appear well formed upon a few stupose stalks, the drop is covered with a concave glass, which should not touch it, but which, however, should be sufficiently thin and low to permit it to serve as a strong lens.

f. In order to prevent the concave glass from being dimmed on the lower surface by the evaporation of the water, its temperature and that of the lens carrier are kept a little higher than that of the object carrier during the operation.

It is simpler and more convenient to observe without the cover when there is no likelihood of dust. Thus the drop is covered only after each observation.

g. The object carrier is next placed on a lightly compressed cushion of wet rags, which rests on a flat body, impermeable to water. It is all covered with a bell glass or a goblet.

h. The humidity under the bell glass is easily maintained, even at a temperature of 25 to 26° C. (about 20° R.), by moistening the little cushion once or twice a day. In this way, there is need of replacing the water in the drop under observation only once every 24 hours, even supposing it to be very small. Thus, uncovering it too often is avoided.

i. It can further be preserved intact for whole months without adding water to it by placing the object carrier, covered by a watch glass, in a screw-capped tin box half full of wet paper or rags, which is filled to the top with slightly moistened paper. In this way it is possible to transport this box wherever desired, provided the drop be very small, without the latter being deranged, and without running the risk of its becoming desiccated.[7]

42. By following the observations for a sufficient time, then, it is very clearly seen that the tufts of the stupose stalks dilate and spread out, and that their leaves extend considerably in length. These leaves behave in their development like the naked stalks (15 and 17). The part next to the gemma or the principal stalk is much the most transparent: in it are distinguished some sort of elements or cells, the transverse walls of which take the form of granules, which at times become rather distinct globules (§23). The same thing happens to the filaments of the aigrettes (Pl. 1, fig. 35); however, these latter do not elongate so much,

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[7] These details may appear to be minute, but I have thought them necessary in order to avert precipitate judgments concerning the experimental results, which are delicate and difficult to obtain in a very clear manner. Moreover, they are applicable not only to bunt, but to a very large number of microscopic plants that they provide the means of observing more easily.

and sometimes the blades or little leaves, of which they appear to be composed, are transformed, without noticeable extension, into a continuous train of globules of a diameter that slightly surpasses the width of the leaf. This sort of development is very rare; but we shall see (sec. VI.) that it has a marked analogy to the fructification of several other internal plants.

The parts of the aigrettes often are detached, and are not prevented thereby from producing granules or globules.

43. It is easy to prove now that *the bunt plant begins its growth on the wheat plant, or in its vicinity, and not within it.*

For (1) I have observed on young wheat plants started in soil in which I had mixed a great deal of bunt, and had kept very moist, several aigrettes and several stupose stalks, some of which bore their fruits, or growths that resembled them; and (2) one cannot imagine that the mature gemmae of bunt enter the wheat plant; for, although they are very small when compared to the seeds of the best known plants, they are, nevertheless, still far too large to be admitted into the openings this plant can offer them.

44. It is, moreover, easy to prove that *the stalks, the branches, or some of the growths of bunt penetrate in one way or another into the wheat plant while the latter is still very young; that they arrive at the seat of the embryo of the seed, where they produce globules that enlarge with the [wheat] head, and finally become for the most part perfect gemmae when the wheat approaches maturity.*

For, first, the bunt gemmae can begin growth only in the open (23); and, second, we have seen in paragraphs 34, 35, and 36 that gemmae of all sizes, from those that appear as very small granules or globules to those that have acquired their maximal volume, are encountered in very great numbers in the embryo of the kernel, in heads still very far from emerging from their coverings: therefore, *these very small globules or the plant that bears them, that is to say, the stalks that the gemma produces outside, or some of its branches, passed into the embryo of the seed, while the wheat plant was still very young.*

45. That these little globules, which are nothing but very immature bunt gemmae are produced outside and then become scattered; that, disseminated by chance, a small part enter the plant through the roots; and that, circulating with the fluids,

the small number of them that reach the embryo enlarge and mature there: one feels, withal, how improbable such a supposition is, and it will later be seen that it is in little accord with experience.

46. Thus, although I have never been able by direct experiments to assure myself of the existence within the wheat plant of the ultimate ramifications of that of bunt, doubtless because of their excessive tenuity, which permits them to be perceived only upon a very clear glass, it is, nevertheless, most probable that these ramifications penetrate into this plant, and insinuate themselves into the embryo, where they fructify, and where the seeds mature.

47. We have seen that at the bottom of the water the bunt gemmae produce only naked, somewhat etiolated stalks, and that the aigrettes and the stupose stalks, which are beyond a doubt the result of a more vigorous growth, arise only from gemmae that germinate at the surface of the water or on moist bodies. Moreover, it can be proved by direct experiments that these latter developments can take place only in the open air,[8] and that otherwise there are produced only deformed stalks, which do not grow, or etiolated stalks. Thus, it can be only the gemmae that are at the surface of the soil, or very near the surface, that serve to propagate bunt.

48. This plant is exceedingly fragile and delicate; it would quickly be destroyed by rain, wind, and other incidents, from which it can escape only by the protection of the wheat plant, into which it is consequently necessary that it penetrate early. The gemmae, moistened by dew, germinate on this plant even

[8] This need of air, which appears at this stage, relatively to the volume of the plant, much more imperative than for the majority of other plants, diminishes considerably, if, indeed, it does not become absolutely *nil*, at the time of further developments, which probably should in the natural state take place in the interior of another plant, where the air penetrates only indirectly. If in a tumbler, simply covered with paper, in which the bunt is beginning to produce aigrettes and stupose stalks at the surface of the water, one takes some of these stalks, with gemmae beginning to germinate and others not yet germinating, when he puts them in a drop of water upon the object carrier, and covers them tightly with a concave glass, he will find, after a few days, that only the stupose stalks and the aigrettes already formed have continued to develop, whereas the rest have remained in very nearly the same stage, although the temperature was sufficient for the development of all or nearly all of those in the glass at the surface of the water.

when it is beginning to come up, or on moist soil, some days after the germination of the more deeply buried wheat; and when it emerges, the aigrettes are formed, and the stupose stalks are about ready to expand. It is then probably the shoots of the aigrettes, or those of the stupose stalks, that are destined to enter the wheat plant.

49. The dew, it is true, often dissipates during the day, but the bunt is not prevented from developing, by having several times been successively moistened and dried, provided, however, that this does not occur at a very high temperature, or too near the time when growth begins.

50. The bunt plant, under natural conditions, usually is destroyed after a few days; yet the branches that have penetrated the wheat plant keep growing: thus, in our observations on a small scale, we see the ultimate ramifications of the plant continue to grow, though the gemma be empty, or even when it is entirely destroyed. Moreover, the best known plants offer many analogous cases.

51. As to fructification, its abundance in the bunted kernel hinders observing there the arrangement of the gemmae. We shall soon see what analogy permits to conjecture in this regard, according to what happens in the case of some other internal plants whose history I shall sketch.

52. Meanwhile there follows in summary that of bunt:

From a gemma, which one may consider as a fragment of the plant, and of which the exterior coat, which is brown or yellow and vermiculate, cracks and is parted after a long sojourn in water and exposes another, transparent and colorless, arises a stalk, at first enclosed in a sort of sheath or case, which it soon pierces, producing at its extremity a pad composed of small opaque grains, each of which becomes a stalk similar to the first, enclosed as it is in a sheath, but a smaller one. This latter stalk sometimes branches, from which one may presume that each of these secondary stalks, in favorable circumstances, might produce (as I have sometimes seen, though in fact very rarely), as well as the mother stalk, a pad composed of granules, each of which would become a ternary stalk, etc.

53. Thus this plant is a true vegetable hydra, comparable to the hydras or polyps of Trembley; and what renders the re-

semblance as complete as it can be from one kingdom to the other is that each stalk, the principal one as well as the secondary and ternary ones, is composed of elements or parts that, becoming separated, mature, and probably form anew gemmae entirely like those whence they originate, etc.; in the same way as each grain or each perceptible fragment of the animal hydra is capable of reproducing a polyp exactly like the mother polyp.

54. In fact, several principal stalks that I have called naked, push their sheath ahead without piercing it, and elongate without branching; but this difference is only accidental, and depends upon their having continually grown under water: it is a kind of etiolation, comparable to that produced by deficient light in the case of other plants; and such a naked stalk would have assumed one of the two other forms, had circumstances been different.

55. As to the development of the aigrettes, it is nearly the same as that of the stupose stalks, with the difference that the latter are more extended, and, what is very noticeable, that they nearly always bear growths that have the appearance of small fruits, while one very rarely sees any of them on the others. Nevertheless, it is possible that these differences may not be other than accidental; it is at least certain that when the newly formed liliaceous stalks are immersed in water, or laid on its surface, they develop a little differently. (See the figures and their explanation.)

56. But, since it appears that the ultimate ramifications of the plant or, to speak more exactly, the whole plant, forms its fruits or gemmae, which, in a way, are only parts of it, the question arises as to what are the growths that the stupose stalks bear, and of what use are they?

Would they really be fruits? But they would be too large to be capable of penetrating *en masse* into the wheat plant, and they would not be abundant enough for the granules, which can sometimes be observed in them, to be able, in passing into this plant, to form the fructification of bunt, unless these fruits are very highly regenerative, like those of several other plants, or like the *eggs* of certain polyps, each fragment of which is equally capable of reproducing a new polyp.

As these little bodies are quite similar to anthers or even cat-

kins, and are scarcely ever found except on stalks a little different from the others, I have thought that they might be male organs, which would sufficiently explain why these stalks grow more than the aigrettes.

These growths might be parasites of bunt, or in a way, parasites of the second order.

There will be found, in the following section, the history of several growths of the same form, or nearly so, which seem to have some analogy to these, that may throw some light upon their nature.

57. I have said (48) that the bunt plant must early penetrate the wheat plant, in consequence of its delicate nature and of the necessity that it begin its growth in the open air (47). Moreover, I prove it by direct experiments:

(1) I scattered bunt, either mixed in some pulverized earth or suspended in water, upon wheat plants of different ages, from the time when they had emerged from the soil only three or four centimeters to that when the majority of the leaves had developed. I multiplied these experiments, and I made them upon large enough quantities of wheat for their results to be decisive: they did not yield more bunt than the average taken with much care in the same field, where, in general, there was very little.

(2) At the time when the heads began to appear, and in a field where some of them were bunted, I noted about 50 that were not, but the plants of which appeared to have the characters of those that should bear that kind; however, none became bunted.

(3) At the same period, and in the same field, I threshed with some of the bunt of that year several hundred heads taken at random, of which only one later was bunted.

(4) In one experiment in which bunt was scattered upon the soil immediately, or very soon after the wheat was sowed, there were many bunted heads.

Thus, *it is only at the time of germination, or a very little while after, that the introduction of the bunt plant into that of the wheat occurs.*

58. After all that precedes, there can be no doubt that the bunt plant is destined to pass a part of its life in the wheat plant, and to fructify in the kernel. It may be regarded then as an internal plant, and be compared in this regard to the internal

worms of animals. The number of species of internal plants should be much greater than of other plants; at least it has appeared to me until the present that each of the latter is subject to attack by several of the former, and that the internal plant that attacks one species is not adapted to attacking another.

VI

Some Other Internal Plants Whose History May Serve to Throw Some Light on the Fructification of Bunt

The Uredo or Rust of Wheat

59. On the upper surface of wheat leaves, as early as the month of April, there are found, as is well known, small, yellow, oblong or linear tubercles, which, becoming constantly more numerous, often end by uniting, and by entirely encrusting the leaf. They also are found, but very rarely, on the under surface. Sometimes to a yellow tubercle on one surface, there is a corresponding blackish one on the other, containing some of the little club-shape bodies called *puccinias,* of which I shall soon speak. In this country the effects of this disease, known under the name of *rust* (*uredo lineaire,* de Cand.), are ordinarily confined to this: it does not seem to have any influence on the seed.

These tubercles are formed by the swollen epidermis. They receive their color from the powder with which they are filled. This dust, which is scattered abroad when they burst, appears under the microscope to consist of globules larger in general than those of bunt, some of them having double the diameter. After 40 or 50 hours in water, at a temperature of 20 to 25° C. (16 to 20° R.), they put out large, long, jointed stalks, but on none of them have I yet seen either aigrettes or stupose stalks.

60. In the month of July, I found, on the leaves and chiefly on the culms of a wheat plant from seed I had sown in March and April, a rust that might be of a different species from that of which I have just spoken. I believe it is this late rust of which Sir Joseph Banks has given a description, which I know only from a few words reported in *Bibliothèque britannique,* and from what I have been able to gather from the conversation of a naturalist scholar who has read the memoir. M. de Candolle, in the

new edition of *Flore française* has sketched, in part, the history of its fructification, under the name of *puccinie des graminées.*

I had observed it for a long time on wheat culms, the seed of which, purposely infected at the ordinary time of harvest, had yielded heads for the most part bunted; but I had not recognized it as a rust.

It was manifest only as short, velvety streaks filled with puccinias. I have since found some globules among the puccinias in similar spots, which affected the plant only very slightly.

61. But the epidermis of the leaves and stems of wheat sown in March and April was raised or torn in a thousand places by tubercles of very irregular length, and of a half millimeter in width. Some of them were black, others the color of iron rust. The first form a rather hard sort of crust; the others are covered with a dust, under which is again found the black crust, or a black spot, which the cracked or split epidermis partly covers.

62. These tubercles are filled with puccinias or with slightly compressed oval spheroids, different from those of the preceding rust, which are spherical. Sometimes the puccinias and the spheroids are together in the same tubercle; sometimes it is filled with only one or the other. The former ordinarily are found in the black tubercles; the spheroids form the rust-colored dust.

63. The shape of the puccinias varies much according to age or circumstance. The youngest or the smallest comprise a colorless, transparent, oblong mass; a little more advanced, they are less transparent, as if embroidered on the surface, and borne on a kind of stalk or pedicel, which often is flattened, transparent, and in general of the same length as themselves. Others bear a larger or longer body, formed of a cylindrical sheath or pericarp, terminated in a blunt point, enclosing one or two spheroids, rarely three; one of which, the least distinct, is borne on the pedicel, and the other, above this one, is better formed but nearly always a little oval or elongated in the direction of the axis of the sheath.

64. These spheroids, in enlarging, distend the walls of the pericarp, which then appears constricted about the middle, or it forms there a circular section, at first very narrow, which continually enlarges, so that the upper spheroid appears to be coifed with a sort of cap and lodged in a cup formed by the lower part

of the pericarp that encloses the other spheroid, and extends beyond it more or less.

65. The upper spheroid is finally detached from the rest of the spindle, and the same thing happens, but later, to the lower spheroid. The latter often aborts, and the part of the pericarp containing its embryo remains empty. A sort of appendage or stem is sometimes seen attached to the upper globule, which the latter often retains when it is detached. This stem, which resembles the pedicel of the spindle, might give the impression that the globule was beginning to become spindle-shape; in fact, it differs little then from less advanced puccinias or from those with only one locule: but the details into which I have just entered, and those that will follow, prove clearly that there is nothing in this, and that the growth of this plant follows a course contrary to that which one would assume to be necessary. This appendage is probably only débris from the aborted spheroid, or some remains of a coat common to the two spheroids.

I did not follow these different developments of one individual puccinia, which would have been very difficult, since they operate only on the living plant attacked; but several times I did observe all their gradations in a large number of individuals that I had under observation at the same time.

66. Some of these spheroids, or globules thus detached, are partly transparent. A small opaque ball, granular and very distinct from the rest of the globule, is then seen in the middle. This, after being for a few hours in water at a sufficiently high temperature, and after having pierced the oval spheroid perpendicularly to the long axis, develops into a stalk that ordinarily is distorted like the tendril of the vine.

67. This stalk, sometimes falling and flattening out on the object carrier, assumes a branching form, or appears to be provided with antler-like structures; which is often only the effect of the spirals of the helix, which lie upon one another. But it also happens sometimes that the stalks are really branched, and resemble trees stripped of their leaves and small branches.

68. The puccinias in the black spots often are found aggregated in balls of 100 or 150 on a rounded mass where they appear to be rooted by their pedicels, with their apices outside, so

that the whole grossly resembles a head of globe thistle (*sphaerocephalus* or *retro*).

69. It is rare that this arrangement is naturally presented to the observer; but it can be provided. To accomplish this, one lifts a very small scale from a black tubercle with a penknife, places it in a drop of water on a flat glass, and lays another glass on it, pressing a little. Then the little black mass, which at first appeared confused, divides into distinct parts such as we have just described. Upon some of them, puccinias of all ages are seen; and the youngest are also sometimes found ranged parallel on an elongated placenta, like the pips of the pumpkin or melon on the flesh of these fruits.

70. Here, then, in a few words, is the history of this internal plant: the gemmae put out a stalk that enters that of the wheat plant, probably during germination, or shortly after. It produces under the epidermis, or near the epidermis, umbel-like structures composed of puccinias, still extremely young, which appear to be nothing other than the tips of the stalks of an aigrette, whose pedicels are united at the base in a spherical head or receptacle. These young puccinias become little by little club- or spindle-shape pericarps of two locules, each enclosing a globule (gemma or spore). Sometimes there is only one locule; sometimes, but rarely, there are three or even four. These globules become detached as they mature. Their accumulation forces the epidermis to rupture in order to free them, and the rest of the spindles mature or become desiccated, accordingly as they are more or less advanced, or more or less exposed, in forming black spots on the stalk. In favorable circumstances, the globules, in their turn, put out stalks similar to those from which they arose, etc.

71. I have not read Sir Joseph Banks' memoir; but, judging from what I have heard of it, its illustrious author thinks that the globules of the rust are the first stage in the life of the plant, and the puccinias or spindle-shape bodies the second. If the observations I have just reported are exact, it cannot be doubted that the globules are a growth posterior to that of the spindles.[9] These latter, however, would not then be the first stage of the

[9] Sometimes the puccinias take the form of a spindle.

plant; for the first is that of the stalk that emerges from the globules: but they should be the first stage of its fructification.

It is true that the puccinias[10] in general appear outside only after the globules, and that thus they appear to be the second stage of the plant; but this is only an appearance, and the globules were, none the less, originally enclosed in the puccinias.

The Uredos of Couch-grass, Asparagus, and Garlic

72. One finds on the leaves of couch-grass an uredo, whose pulverulent tubercles are a reddish brown, the globules spherical, and the pericarps, smaller than those of the preceding uredo, generally more distinct of pedicel. The one kind are slender and terminated by a cylindrical or conical appendage; the others are large, short, and, for the most part, rounded or globular at the tip, doubtless because the upper part of the coat has fallen away.

73. The uredo of asparagus attacks alike the stems, the leaves, and the fruits. One finds beneath the epidermis, in inconspicuous tubercles, a clear brown dust that becomes detached when the tubercle opens, and exposes the puccinias. This dust, unaided, scarcely ever appears outside, and it is necessary to raise the epidermis to find it. It is composed of spherical globules. The puccinias are larger than those of couch-grass. Moreover, these two internal plants resemble each other more than the uredo of couch-grass resembles that of wheat (60–71); which is quite remarkable, in view of the differences of the genera.

74. I found on the leaves of garlic an uredo, whose globules produce in water, in a short time, stalks that resemble hartshorns, and that become very long. In general the tubercles are yellow: more rarely black ones are found, formed of puccinias with two locules, larger than those of wheat, but whose history, however, is nearly the same. They are also of very various shapes, and the lower globule often blights.

The Uredos of Bean, Sorrel, and Darnel

75. The puccinias of the uredo of bean, and those of the bilateral uredo of de Candolle, which I have found on common sorrel, have only one locule, and differ from the globules only

[10] I mean here by *puccinia* the pericarp with its pedicel, and I nearly always use this word in the same sense.

because they are pedicellate, and because those of the bean are a little pear-shape. When the latter are very young, they resemble Prince Rupert's drops, the terminal portion of which, instead of being a continuous curve, might be slightly angular.

76. The history of six internal plants that we have just described is, moreover, nearly the same as that of the uredo of wheat.

The Uredo of Rose and That of Currant

77. In the first memoir on bunt that I read to the Society of Montauban, I was inclined to think that the puccinias of the rose were the male organs of the uredo among which they are found. I am now convinced that they are pericarps, and that it is from similar pericarps, but much less advanced, that the globules of this uredo come. One finds, in fact, on removing the orange-color dust from a tubercle, that the substance of the leaf immediately below, viewed under the microscope, and lightly compressed between two flat glasses in a little water, is divided in small, rounded heaps, composed of curved transparent tubes, the youngest of which are nearly colorless. They later become reddish yellow, and swell towards the tip. Small, sparse, irregular grains are seen there. Soon these small tubes become erect; the internal grains appear more distinct, better ordered; the swollen part continually draws nearer to the form of the cylindrical top of the mature puccinia; but it is still transparent, and within may sometimes be seen very distinctly as many as a dozen globules ranged in file, one after another, and of nearly the same color as the mature globules. The puccinias take on color, rise, and become more and more opaque, until finally the pericarp is completely black, and in the condition in which it is seen externally.

78. The uredo of the common currant (*ribes rubrum* L.) much resembles that of the rose, and behaves nearly the same. The tip of the pericarps is rounded; the layers are better marked, there being only three or four of them. Several puccinias, some time after beginning to form, take the shape of a Prince Rupert's drop; others are curved in a semicircle, like those of the rose; but here one notices, in addition, one or two swellings, besides that of the tip. In the more advanced ones, those about to take the form they should retain, one observes only four or five globules

that are most often perfectly distinct, although smaller than those that are detached or become detached. A few puccinias also are seen here with a single locule, or pedicellate globules similar to those sometimes encountered in the wheat rust (65).

Reflections on the Internal Plants Whose History We Have Just Sketched

79. It does not suffice for the globules enclosed by the puccinias that are dried in the air, as it does for those that they emit under the epidermis before their coat is hardened, to remain a few hours in water, in order to produce stalks. There also are detached globules that do not germinate at all or germinate with difficulty; and it may be conjectured with much probability that they have come from too far advanced puccinias. One finds them, in fact, principally in the black tubercles with the dry puccinias.

80. A question, therefore, here arises that we shall discuss before going farther.

Of what use are the puccinias that come to maturity in the open air, and occur sometimes in very large proportion among the uredos?

To answer this plausibly, if not wholly satisfactorily, it is necessary to enter into some preliminary details.

81. Since the gemmae of the uredos put out stalks, and as their fructifications are found under the epidermis of the plants they have attacked, and their progress can be followed from their origin to the time when new gemmae are reproduced and detached, it cannot be doubted that these stalks penetrate into the young plant, or, if it is a tree, into its young shoots, while their delicacy permits.

82. But the gemmae of the uredo of the rose, which I shall take here as an example, being scattered upon the ground or upon the branches in the autumn or at the end of the summer, or falling with the dried leaf, are unable to delay germination, and consequently their plant should be destroyed quite a long time before it will be able to penetrate into the young buds, which appear only in the following spring: How then will the reproduction of the parasitic plant take place?

This new question is bound up with the first, the solution of which it presents along with its own.

83. The backward gemmae and the puccinias probably supply it. For some of the latter, without doubt, a considerable time is necessary for the emission of the globules that they contain, and for these globules later to germinate. It is quite possible that a few months do not suffice for certain of these, but that one or more years are necessary.[11] This will be confirmed, I think, by some experiments I have attempted on this subject.

84. The globules or gemmae that require only a few hours for germination are not for that reason without use; they serve for the reproduction of the uredo in the plants whose young shoots are renewed or succeed each other several times in the same season.

85. Besides, it is quite possible that the uredo, once introduced into the trunk or the branches of the tree, lives there until the death of the latter, always ready to fructify under the epidermis of the leaves, the stems, or the fruit; that it is fixed here, in a way, like the tree is in the ground by its roots.

86. The uredos with black dust, which have been called *smut* or *blight*, present some phenomena analogous to those of bunt and the uredos with yellow, orange, or brown dust. They are certainly internal plants. The one that attacks wheat does not renew itself at all in the same season, as Bulliard appears to have thought. The dust of this uredo is composed of gemmae for the most part much smaller than those of bunt and of the rusts. In water they put out naked and simple stalks, though rarely double or triple ones; at other times the gemmae that float at the surface of the water produce there aigrettes or series of globules; but all these are much smaller and, consequently, still more difficult to observe than in the case of bunt or the rusts.

87. One finds the blight or smut of wheat in much younger heads, or those much farther from maturity than the heads that bear bunt. One of them viewed through a magnifying glass and

[11] It is thus that the butterfly of the liveried caterpillar (and without doubt several others) in summer lays eggs that, as I have observed it, do not hatch until the following spring, at a temperature much lower than that they then experience; because they are covered by a varnish that retards the evaporation of the humours and can be dissolved only by the winter rains.

under the microscope is represented in Pl. 3, figs. 6 and 7. The interior is filled at this stage with little rounded groups (Pl. 3, fig. 8), which I at first took for heaps of gemmae, but which are very probably composed of puccinias with a single locule, analogous to those of the sorrel.

88. As these puccinias are very small, it is difficult to distinguish them from true gemmae; and it is because of this that one often thinks he has found some of the latter that do not germinate, or that germinate only with difficulty, whereas in the same species others require only three or four hours for germination.

89. These puccinias or backward gemmae are necessary to several of these uredos. Often carried in the air before the maturity of the seeds of the species that they attack, they are unable to attach themselves to them, and only rarely find themselves in position to penetrate into their seedlings when they themselves germinate. The seeds must, therefore, be capable of living for a greater or less period upon the ground without producing stalks, in order that some of them may always germinate at the time of germination of the [host] plants in which their fructification should take place.

90. Although I have yet to speak of certain uredineous growths, since they present peculiarities, some of which may cast doubt upon their vegetable nature, I shall stop yet a moment to consider the similarities of bunt to the uredos that we have just described, especially those that relate to their fructifications.

91. Everything leads to the belief that the fructification in bunt is entirely analogous to that of the uredos; since the tips of the stalks or of the aigrettes which have penetrated as far as the seed become filled with globules which enlarge and pass out of them, but they are so abundant and therefore so compressed and their coats are so delicate that they cannot be distinguished there.

92. As to the mature puccinias, they would become useless; for, (1) the bunted kernels correspond to them, as we shall see; (2) the gemmae of bunt, in general, require much more time to germinate than those of the uredos; and (3) bunt becomes attached to the kernels, especially to those that bear a small tuft on the end opposite the germ, which so orients its stalk as to penetrate the wheat plant when it germinates.

93. Since bunt attacks the entire mass of the kernel, one might think that there is more difference than really exists between it and the black- or brown-dust uredos, which scarcely attack more than the epidermis of the plants.

But, in addition to the uredos' often attacking the entire thickness of the leaves, like *bifrons*, and their globules and puccinias sometimes being found in the stem itself, one quite often sees heads of wheat formed of very elongated empty pistils or ovaries (Pl. 3, figs. 3 and 4), some of which are lined internally with confluent tubercles filled with bunt. This proves that bunt develops on the inner walls of the pistil, as the rust of wheat develops on the chaff or *glumes*, which have been regarded as petals, and that they accumulate there to the point of filling the seed, and excluding the flour from it.

Some Uredos or Uredineous Growths with White Dust

94. *a.* On the under surface of leaves of mountain spinach (*atriplex patula* L.) may sometimes be seen very unequal and irregular, whitish or yellowish spots, which appear to be a little hollow and sometimes resemble arborizations. The yellow tint penetrates the whole leaf, and the corresponding places on the other side are slightly raised in humps. They bear somewhat cottony or velvety, gray tubercles, speckled with very small black points.

b. Under the microscope the cottony tubercles resemble copses of dichotomous shrubs, bearing at the tip of each of the ultimate branches globules of various sizes, the smallest of which are transparent.

c. These shrubs are fistular, extremely sensitive to the influences of humidity or of dryness; so that, if one blows a little moist breath on the leaf in which they are implanted, they each make two or three turns upon themselves, instantly turning again to resume their original status. The trunks of some of them appeared to me to be formed by a sort of ribbon rolled in a hollow cylinder like a twisted cannon.

d. When they are detached, and moistened in the same manner, they become filled with water while being twisted, and release it an instant later through the base of their trunk, carrying with it a few very small particles of a substance, whose nature it would not be easy to determine.

e. However slightly apparent the yellow spot is on the upper surface of the leaf, some of these little shrub-like structures are nearly always found on the corresponding points of the under surface; sometimes also they occur as isolated and scattered gemmae that, moreover, do not appear to have been brought there; at other times, finally, the place corresponding to the yellow spot appears to differ from the rest of the surface only by the color, which is always a little less perceptible than on the other side.

f. The globules fall when their size nearly equals that of a gemma of bunt. They are ovoid and slightly transparent. After a few hours in water at a slightly raised temperature, they put out dichotomous stalks that nevertheless differ from those from which their seed originate, probably because of the difference in the media in which they have grown.

g. The opacity of the globules lessens in proportion as the stalk grows, and they become absolutely transparent; so that it cannot be doubted that they originally contained the embryo of this same stalk. As we have seen, a similar thing happens, in a more or less marked manner, to the gemmae of bunt and uredos.

h. I have found this plant upon *chenopodium vulvaria* L. and lettuce. The latter is a little different. The spots it produces upon the leaf have a violet tint. The shrub-like structures are larger; in some of them the trunk is divided first into three branches instead of two, each of which then subdivides into three others, etc. Their ultimate branches are triangular; three, four, or five of them, united at their base, seem to form a small, star-like receptacle for each seed.

i. Is this plant internal, and should the alteration of the leaf be attributed to it; or, indeed, is it an external parasite which thrives because of this alteration occasioned by an internal plant?

In support of this latter opinion, one always finds in the thickness of the leaf, in the places where it is spotted with yellow, globules or tubercles of various sizes, which are perceived by placing it between the eye and the light, and which belong to a group of plants that are certainly internal. The latter are not uredos, at all, but nearly always accompany them. We shall speak of them below.

k. One would be tempted to place the small dichotomous shrub-like structure in *botrytis;* but it will be noted at least that each of the globules it bears is a spore, and not a capsule (*f.* and *g.*).

95. *a.* The leaves of purslane (*portulaca oleracea* L.) sometimes bear on their upper surface round tubercles, of a slightly reddish white, quite shiny, a line or two in diameter, very unequal, often irregular, not a little resembling the smallpox pustules. They begin as small white spots that gradually increase in extent and thickness. The leaves on which they abound are often hollowed like a spoon; they dry and fall before the others.

b. The raised epidermis breaks after a time, and reveals inside a white dust, which, observed under the microscope, shows irregular polyhedric fragments, trunks of pyramids or cones, which sparkle like crystals in reflected light. In transmitted light, they appear almost black; only the luminous parts are so distributed that several of these polyhedrons resemble gratings, and the truncated cones seem hollow.

c. When they are moistened, the majority take the form of an egg depressed circularly in the middle of its length, but almost imperceptibly, and having the small end much less rounded than the other. Some of them remain irregular, or become globular; these are the smallest. The former have a length of one and one-half times the diameter of the gemma of bunt, and a breadth of a little more than one diameter.

d. An hour or two after immersion, sometimes only 40 or 45 minutes, doubtless according to the temperature, which, while I have observed them, has varied from 12 to 16° R., the larger and more convex extremity opens so that the whole resembles a bottle whence a good part of the neck has been removed.

e. Very soon one globule appears on the outside, immediately followed by three, four, five, or six others, which come together in an instant in a ball, and move together for some time, the ball balancing or turning horizontally upon itself, or rolling in the liquid.

f. Ordinarily, the globules then separate, but sometimes they all remain together in such manner that one would not suspect, if he had not seen others like them, that this was an assemblage of several individuals. Sometimes two or three of them travel together, directly in contact or held by a sort of bond.

g. Those that separate, and they are the majority, are sometimes a little angular or tubercular, and, if I am not mistaken, hollowed like a boat, which makes them in certain situations appear to contain another globule. They resemble *bursaria;* but are very small, their diameter being only half or a third that of a bunt gemma.

h. They move like balls, but with much more agility. They leave no more doubt of their animal nature than do the majority of the animalcules, called *infusoria;* and I confess that I no longer believe that the latter originate wholly in the air, that is to say, that their germs, as has been thought until now, following too generalized observations, pass from the air into the infusions in which they are found. It will be seen that I have for my opinion still other reasons than those that may be deduced from the facts I have just given.

i. Little by little the movement of these globules slackens; they become fixed sooner or later, either at the surface of the water or at the bottom. The latter settle, enlarge, wrinkle, rupture, and assume the appearance of a formless mass of granules. This alteration doubtless arises from their extreme delicacy, which scarcely permits them to touch a hard body without being broken. Be that as it may, such has been the fate of the majority of those that I have observed. Others have maintained their globular form long after they have ceased to move. Still others, finally, and these are ordinarily the ones that come to rest at the surface, become a little larger and less transparent, which may perhaps be only an illusion due to emersion, which always tends then to augment. Next they put out a small, slender stalk, slightly twisted or undulated, articulated or granulated, at the end of which is formed a globule a little smaller than the animalcule that produced the stalk. Finally, this globule elongates into a body which assumes nearly the form of the pod or dry berry that contains the seeds of *capsicum annuum* L. The entire stalk then has a length equal to 6 or 7 diameters of a bunt gemma. This kind of animal growth stops there, at least within the temperature of 12 to 16° at which I have observed it.

k. I saw on cabbage a very similar growth that presented some like phenomena. The pustules are white and smaller; but they are often united in large numbers in irregular and quite

extended patches. The globules containing the animalcules become more generally spherical in water. These animalcules are a little more angular, especially at the time when they separate. Instead of becoming larger when they cease to move, it has seemed to me that they were smaller then. They nearly all sink to the bottom of the water when they die, but without becoming separated into granules, as those of purslane; at least that happens only to a small number. The stalks that they produce are more curved and more granular; when they reach the air, in growing, they terminate in a very irregular, elongated body. The animalcule, when this type of growth is advanced, is nothing more than a globular sac, so transparent that it is scarcely perceptible in water. On the interior is seen a more distinct and much smaller globule, which diminishes in proportion as the stalk grows.

l. The leaves of salsify also offer a growth of the same nature, which has been taken for an uredo. The globules containing the animalcules are a little larger and longer than in the cabbage and purslane. I have not yet been able to see these animalcules put out stalks.

m. In repeated experiments the emission of moving globules of the purslane has rarely failed to occur. That of the salsify on the contrary rarely succeeds, without my yet knowing why. These globules, however, always pass out after a certain time, but dead, or nearly without movement, often deformed, or several united in a single mass. The still-born globules of purslane do not produce stalks at all, any more than do those of salsify.

n. There is found on the two surfaces of the leaves of *amaranthus blitum* L., more often on the under surface, still another growth similar to the last three, with some slight differences, but from which I have so far seen only a very small number of living animalcules pass out. These put out no stalks at all. The tubercles that contain the white dust are much smaller than in the preceding species.

o. If one lightly scrape the bottom of some tubercles of purslane, amaranthus and salsify,[12] especially those of the last, and place the detached substance in a drop of water, he observes there

[12] I have as yet seen this growth in the cabbage only upon a piece of nearly dry leaf.

subcylindrical or conical pericarps, which open longitudinally, and sometimes expose two or three globules as large as gemmae of bunt, one of which is ordinarily ready to pass out. These globules differ from those that constitute the white dust, and that contain animalcules, only because they are younger.

96. Besides the moving globules just considered, a group of small bodies, apparently of vegetable origin, have very decided movements, though rarely so lively as those of these globules. I shall give some examples:

a. I saw a globule of the linear uredo, which had put out a large short stalk, shoot into the water an infinite number of excessively small corpuscles, which moved with great vivacity, nevertheless without much change of position, that is to say without going far away in a short time from the vicinity of the globule, but oscillating, going back and forth in short straight lines, or describing very eccentric little orbits. This phenomenon, which I have observed at other times, is very rare, and is doubtless only an accident. These kinds of monads can be found in gemmae, like certain larvae in fruits, without belonging to them. But the animalcules of purslane and of other plants that bear similar ones, are there too constantly and their course is too regular to permit the supposition that the same is true regarding them.

b. If one lightly touch the bottom of a drop of water, upon a glass, with the end of an ergot of rye or some other gramineous plant, he sees in the microscope ovoid, transparent corpuscles that, at a slightly raised temperature, soon take on very decided movements, although less lively than those of animalcules called *infusoria*. They also produce stalks (Pl. 3, fig. 9). Then their movement slackens; but sometimes the corpuscle and the stalk that it has produced still move together. The interior of the ergot contains much longer corpuscles of various forms, which appear to be composed of several of the former, and also move as they do, but less perceptibly.

c. I shall report, in a memoir that will soon appear, the means by which I assured myself of the spontaneity of these movements and of those of several other particles that are vegetable in appearance. Meanwhile, I shall give here a summary of my observations on this subject.

d. The seeds or gemmae of a great many microscopic plants, and perhaps of all of them, when they are sufficiently small and rounded, move perceptibly at a high temperature, whether at the bottom or at the surface of the water; but their movement is sometimes very feeble, and some precautions are necessary to perceive it.

e. Then, when they are crushed in water, particles of various forms and different sizes nearly always come out of them, the majority of which have a very decided movement. The gemmae of uredos and of bunt are of this category.

f. One observes, in a drop of water in which he has diluted a little of the interior substance of any kind of seed, of a kernel of wheat, for example, a large number of very small moving particles.

g. The same thing is observed in the juice from the root, the stem, the leaves, the flowers or the fruits of various plants.

h. The anthers of the stamens contain, as is known, globules that constitute the pollen or dusts. These globules are of various forms and different sizes. In water, their movements are like those that have long been noted, and the cause of which, ordinarily easy to assign, is purely mechanical. A kind of floating oil is detached from these globules, and some very small, rounded, unequal corpuscles emerge from them. These corpuscles move very decidedly, and it does not appear that any foreign cause can be assigned to this movement.

I find myself here in opposition to a celebrated scholar who honors me with his friendship;[13] and I confess that this would be a grievous presumption against the validity of my observations,

[13] "The anthers," he says, "like the other parts of plants, are nourished probably by juices elaborated in a pad found here at the base of the filaments, the anthers, and the globules. It is certain that the fluid of the dusts changes in proportion as they mature; this juice, at first very thick, becomes constantly more fluid. Bulliard has seen floating in the mucilaginous fluid, some globules or small bodies that seemed to him to be the fluid itself in a concrete form, as in the mallows and in stork's bill; but he did not see any of them at all in the '*dauphins des blés.' These little bodies are without movement.*" PHYS. VÉG. t. II. p. 78.

I think that M. Senebier, in these last words, speaks according to Bulliard, and not from his own observations. Perhaps the little bodies of which Bulliard speaks are not those I have constantly seen leaving the globules of various pollens. He saw only a few of them; those of which I speak are very abundant. Perhaps also this scholar did not see them move, because he observed them in a mucilaginous fluid, or at a too low temperature.

if they were not easy to repeat. I studied chiefly the pollen of the pumpkin, because everything in the flower of this plant is larger than in most others, and consequently easier to observe. When the globules of its pollen, taken from a freshly picked flower, are placed in water, there emerge from it an infinite number of corpuscles, which move at first from the impetus they have received, but continue to move for several weeks, and, consequently, a very long time after equilibrium has been reestablished. In order to restore this equilibrium and remove the causes of illusion, I place upon a flat glass a very small drop of water in which the globules burst. I am careful to have it very pure, and to examine it well in advance to make sure that it contains nothing foreign. I cover this drop with a little concave glass with flat sides, which can be surrounded with oil or wax. These corpuscles enlarge and several unite without ceasing to move, except for a few intermittences, provided the temperature be sufficiently high, as from 20 to 25° R. Sometimes even 12 or 13° suffices.

i. Globules of blood, under the same circumstances, that is to say, in water and at a high temperature, as is well known, have a slight movement like certain very small globules found in the eggs of several insects and invertebrates. They sink in water, and should not be confused with other larger, more unequal globules, which are perhaps nothing but drops of some kind of oil.

j. These various observations seem to prove that all organic particles, or rather particles that are organized or have lately been a part of a living organized whole, are disposed to movement, and that, in fact, they move in water for a very long time when their volume and form permit.

Some Microscopic Plants that Nearly Always Accompany Uredos and Uredineous Growths

97. Uredos are nearly always accompanied by another internal plant, which develops in the interior of the leaf or in the substance of the stem, and externally is manifest as brown, hard tubercles, always covered by the epidermis. One finds in these tubercles very unequal, opaque, reddish-yellow globules, of which the largest have a diameter equal to six or eight times that

of a bunt gemma. When placed in water, these globules burst and expel, sometimes with considerable force, transparent corpuscles, more or less regular and more or less elongated, according to the species. These corpuscles come out in two or three files that form a sort of chain or braid. Ordinarily, they separate quickly. They show a little movement, which is independent of the propulsion they have received, since they maintain it after equilibrium has been reestablished. After a few hours at a high temperature, they each put forth a small stalk. These corpuscles, in asparagus, are very long in proportion to their width; those of *atriplex patula*, which accompany the plant described in §94, are of varied forms, but, in general, ovoid and elongated, and the globules that produce them are green and semi-transparent when young. These globules, opaque in this species and in several others of the same genus, often appear to be suspended in another perceptibly larger and more transparent globule.

98. *a.* One often encounters among uredos small bodies of club, spindle, or pear, or very irregular shape, sometimes resembling puccinias, but of a very different nature, always composed either of several locules or of several distinct globules, unequal, naked, often colorless and transparent, and sometimes opaque. I have been tempted to regard them as the male organs of the uredos. Their origin, which I now know, has long intrigued me. Immersed in water, they quickly put forth from each node or locule branched jointed stalks, which in a short time make a considerable growth when the temperature is sufficiently high.

b. From various points on these stalks arise rows of globules, either simple or forming aigrettes or tufts, or dividing and subdividing into branches like little trees, each branch of which would be a row of globules. These globules enlarge, elongate and divide into segments or into irregular, rounded masses. In a word, they each form again small bodies similar to those that produced the stalks, and from which they arose. They are consequently some sort of bulb or seed with several germs, the last results of the fructification of the kind of plants that have been called *monilias*, but whose history it seems to me, was not yet completely known. It was not known, perhaps, either, that they are capable of completing and recommencing in water all the stages of their growth.

c. However, none of the monilias I observed offered me the complete cycle of their regeneration. I saw it quite clearly in one species that I shall naturally call *solitaire,* because only a single-headed thread starts from one point on the stalk. The globules take the form of a top. The base of the one nearest to the stalk is supported upon the point whence the fructiferous branch starts; upon the point of this one the base of another top-like structure is supported, etc. All elongate a little and divide transversely into three or four segments. These growths sometimes put out stalks, even without becoming separated from the rest of the plant. They are found among the globules of *amaranthus blitum,* which contain the animalcules because they grow on the leaves of this plant, and are mixed with them when the tubercles are opened or when the white dust which one wishes to observe is taken out.

d. I found on the leaves and dry stem of the onion a monilia in all the stages that I have just described; but it differs from the preceding species, principally in that the proliferous branches occur in tufts arising from the same point or nearly so.

e. M. de Candolle describes in *Flore française* three species of monilia. The clustered monilia is the only one I have recognized among those found with the uredos. I had found its bulbs or many-germed seeds on the leaves of lettuce with the plant I have described in §94 *h*. In water I saw them produce stalks that extended to two or three centimeters around the drop in which I had placed them. These stalks produced a large number of proliferous branches, whence a few globules fell after becoming slightly elongated, but did not yet take the form of the *puccinias* from which they arose.

f. As I am speaking here of these plants only on account of the seeming relationship that they have to the uredos, I shall not dwell upon the history of each species; but I shall describe one other of a genus not far removed, because, although it is not an internal plant, its fructification, all the gradations of which it is very easy to see by observing the status of several individuals at one time, appears to me to have the greatest resemblance to that of bunt.

99. *a*. It is manifest on both surfaces of the salsify leaf as a white dust, which covers it entirely, as if it were powdered with

flour. Under the microscope, this dust seemed to consist of ellipsoidal globules, of a length nearly double the breadth, and equivalent to twice the diameter of the bunt gemma. They appeared to be filled with small, rounded bodies of very irregular size.

b. Many of these globules are scattered; others are united end to end in the direction of their axis. These series of globules arise after the manner of the monilias from various points of a sort of network formed by slender filaments that cross each other on the leaf, and several of which arise from the same point. The globules are larger and more distinct as they approach the extremity of each series. Nearer the base, they are marked only by constrictions or slight transverse depressions; lower, by walls separating the cells in which may be seen small bodies of varied form, which are the embryos of the globules.

c. These globules, after a few hours in water at a temperature of 14 or 15° R., put out stalks similar to the filaments from which they originate. They arise from one of the extremities of the globule, a little to one side. Sometimes this germination takes place, although the globules are not yet separated, and one even finds on the leaf some of them that have produced stalks similar to those they put forth in water. These globules are then kinds of gemmae; but I have not yet seen them reproduce in water like the monilias. What makes it doubtful that this be possible is that the filaments seem to penetrate beneath the epidermis; so that when one examines them on the leaf by reflected light, and wishes to follow them in their course, he sometimes loses sight of them, or at least perceives them only with difficulty within a certain space, after which he sees them reappear farther on in the same direction, later to become hidden anew, etc. I do not know whether they draw any nourishment from the leaf; but at least they do not alter it perceptibly.

d. Each series of globules has a common coat, extremely fugacious, such as I imagine invests the bunt gemmae when they form in the kernel, and of which, because of that, one finds no more than fragments or a confused mass, difficult to recognize.

e. This plant differs little from the monilias; but one will separate it from this genus if he considers, among other differences, the ultimate results of its fructification, which are simple gemmae, while those of the monilias are multigerm.

100. In this growth, the proliferous branches, in which at first one distinguishes nothing more than in the stalk whence they arise, except that they are larger, appear later as tubes divided by partitions, or filled with oblong cells placed end to end. In each cell that is somewhat advanced is observed a small rounded body or a little mass of granules, which evidently is the embryo of the gemma. Of these embryos, the one at the tip of the proliferous branch matures and is detached first; the others follow. Each one reproduces a new stalk that gives rise to other proliferous branches, etc. Such is the extremely simple cycle of the regeneration of this plant and many others of the same class.

That of the uredos differs from it only in that their fructification must take place, or at least begin, inside another plant, where most often its progress or different stages can be seen; but the embryos that thrive at the tip of the proliferous branches of the uredos cause those directly underneath to abort; hence, the club shape assumed by the puccinias. Sometimes even, as in the puccinias of the rose, the epidermis of the pedicel, swollen towards the base, opens and reveals, when they are not yet well formed, a series of globules that appear to be the rudiments of another pericarp, destined to succeed the first, or to produce gemmae under the epidermis of the leaf.

Although the fructification of bunt does not appear in such an evident manner, the analogy, or what one is able to observe of it, permits no doubt that it takes place in the same way. From the tip of the stalks ending in the embryo, arise proliferous branches, partitioned, or formed of cells, like the aigrettes, stalks or branches that arise in the open air. Each of the cells contains the embryo of a gemma that matures in the embryo of the seed in proportion as the seed itself matures. These gemmae, as we have seen, are found more or less mature in the embryo of the kernel while it is still extremely young; but the walls of the tubes containing them are so delicate, and these tubes are so crowded in the limited space in which they grow, that they appear only as a confused mass of crumpled, extremely fragile membranes. But the proliferous tubes of the last plant that we described, as well as those of the uredos, forming in open air or in a place in which the walls are capable of yielding or of giving way to their growth or multiplication, whether by expanding or

being torn, assume a form determined by the number of gemmae that thrive at their extremity, and often are even very distinct from their first appearance in the substance of the plant they attack.

101. The fruit-like growths of the tufted stalks of bunt should be analogous to the puccinias of the uredos that succeed in maturing or forming in the open air; but, being unable to get nutriment sufficient or appropriate for their needs from the air, either in the water in which the gemma that produced them is immersed or in that in the body upon which it rests, they abort long before having attained the size they probably might reach if it were possible in these circumstances to provide them with a suitable nutriment. One cannot doubt that the tufted stalks are destined to penetrate into the wheat plant and to go into the embryo to fructify. They probably also would produce there puccinias, properly speaking, were it possible to introduce free circulation of the air there, or expand it sufficiently without disturbing the growth of the wheat. The tufted stalks that grow under our eyes on moist bodies are an image of what would occur then in this latter plant. In fact, this image is imperfect in that the fruit-like growths there would become much larger, and would assume the form of puccinias that become detached from the uredos, and might be able to function as these do. But, as we have already observed (§92), they would be useless, or at least superfluous, in the reproduction of bunt.

102. By all that precedes, I have incontestably established that the immediate cause of bunt is a plant of the genus of the uredos or of a very nearly related genus; that the growth of this plant, as well as that of the majority of the uredos, begins in the open air and is completed within the plant that it attacks. I have given a detailed history of this plant, founded almost entirely upon facts that anyone can easily verify: only a small part of this history is founded upon analogies; but these analogies are so palpable that they are in a way equivalent to observation, and scarcely leave any ground for reasonable doubts.

103. The majority of the plants of this class, which might be included under the general denomination of *internal parasitic plants*, or simply *internal plants*, are capable of making a part of their growth in water, or on very moist bodies. This presents an

easy means of studying them and of proceeding to the investigation of means of preserving useful plants from their ravages. This mode of observation can reveal interesting facts, which, without its help, one would not even suspect; such is the one I have described in §95, and of which I do not think anyone has yet had any idea. This mode of observation will enable us to discover methodically, and without having recourse to costly or cumbersome experiments, whose results, in order to be decisive, sometimes require several years, the surest and simplest preventive of bunt.

VII

Conditions That Inhibit the Growth or the Propagation of Bunt, and Those That Favor Them

104. From what precedes, one easily conceives why bunt is contagious, as several agriculturists and learned naturalists have long since observed.

105. It is further conceivable that in order that the contagion take place the bunt must occur either on the seed or mixed in the soil around it, or, finally, upon the young plant.

106. But this proximity or contact does not always suffice for bunt to be produced in considerable proportion, and the attendant circumstances are not without significance.

107. It is known that if the healthy seeds which are sometimes found upon the same head with the bunted[14] ones are carefully

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[14] M. Tessier regards as an unfathomable enigma these heads that contain "healthy and bunted kernels, and, what is still more difficult to explain, kernels partly healthy and partly bunted." (Art. *Carie*, Dict. d. Agricult. de l'Encycl. méthod. T. II, page 706.) Nothing is easier to find now than the answer to this enigma. Since bunt is a plant whose stalks and their branches invade the wheat stem and fructify in the embryo of the seed, it is conceivable that the number of embryos attacked, and the manner in which they are attacked, depend on the greater or less abundance of the stalks or branches that have penetrated the wheat plant, on their direction, or on causes that favor one of the two growths, that of the wheat or that of the bunt, to the detriment of the other.

The observations of §42 again explain perfectly why the bunt dust placed at some distance from seed sown upon the soil occasions infection.

I shall add that when a certain quantity of bunt dust is sown upon the soil, there appears after several days, especially when the temperature has not been very high, and when care has been taken to maintain a little moisture, a sort of short down, or light white crust, which I used to take for mould; but which, ob-

detached and sown the product is ordinarily healthy wheat. "The good seeds taken from a bunted head germinate and produce excellent plants." (V. Art. *Charbon* de l'Encycl. Édit. de Genève, T. VII, p. 309). This is an experiment I have performed several times, and in which I have always obtained healthy wheat without admixture of bunt.[15]

108. Now, it is true that among these seeds there always are some, about 1/12, that are only partly sound. I have said already that I had succeeded in gathering together some hundreds of them. The germ of these seeds is often intact; some are still capable of producing their plants; and, consequently, in these experiments, they should have produced some bunted plants; for, if the contact is ever established in an intimate manner between seed and bunt, it is in this case, where the latter occurs mixed, in some way, in the juice that must nourish the young plant. However, no bunt at all resulted. It appears then that contact alone does not suffice to establish infection. An experiment that I made long before discovering the cause of this malady might have made one think that it is not contagious.

109. I made some little packets, each containing the dust of several bunted kernels, with one or two sound seeds, the whole closely wrapped in several layers of tissue paper. I lodged them in holes several inches deep, which I made in muddy soil. Each packet gave one or two plants of perfectly healthy wheat.

It was precisely, as will be seen, the precautions that I took to assure infection that prevented it.

110. The bunt that I used was not of the current year; but,

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served under the microscope, appears to be composed of stalks of bunt, of aigrettes, etc., borne on gemmae, the majority of which are empty.

Such then is the bunt plant, which manifests itself to the naked eye, to all appearances, as a mould. This furnishes a simple means of ascertaining without the microscope the effect of preventives.

[15] "From a quantity of bunted heads," says M. Tessier (p. 698), "which supplied me with twelve ounces three grains of bunt, I recovered three ounces of healthy wheat, which I sowed. . . . This wheat, when it had been separated from the glumes, was blackened by the dust with which it had been mixed; it produced two-thirds healthy heads and one-third bunted. . . . "

M. Tessier says further, in accordance with M. Tillet, that healthy seeds taken from half-bunted heads produced a few bunted heads. But he does not say whether these healthy seeds had been separated from the others with sufficient precaution not to have been infected from them, and nothing can be concluded from this experiment relative to those I have made.

although I have since proved that the older it is the less capable it is of germinating, that which I used, being only two years old, could not have lost this faculty. I had no trouble in finding the reason for this singular anomaly when I arrived at an understanding of the immediate cause of bunt.

111. In fact, this disease being occasioned by a plant that begins to grow in the open air, I suspected that air was indispensable to it, and I soon confirmed this conjecture by experiment.

112. I next made several observations and experiments that prove that bunt can grow and thrive only inasmuch as its gemmae are disseminated or separated from one another.

113. I let some whole bunted kernels of wheat remain in water for a very long time, without any of the gemmae that they contained giving any signs of growth, except those to which cracks in the bran permitted free access to the external air. Some other bunted kernels, wrapped in tissue paper and treated in the same way, did not germinate, nor did the bunt dust tightly enclosed in tin foil. However, this same bunt, suspended later in the ordinary way, produced stalks, but fewer the longer it had been enclosed.[16] Thus, the seeds of wheat, wrapped in paper with

[16] For 65 days of the summer of 1806, I kept six kernels of bunted wheat wrapped in tin foil and immersed in common water that was able to penetrate it. The bunt became pale, and, when suspended later as usual, was unable to germinate.

For a long time the tin had borne on the outside, principally on the joints of the leaf, some small, semi-transparent, gray flakes of a light substance that collapsed when it was removed from the water, and resembled mould.

These flakes were formed of small cylindrical, much elongated tubes of different widths. They were gorged with globules with a diameter equal to theirs, pressed tightly against one another, almost like those of pollen in the lobes of the anthers of wheat before their dissemination. The largest were similar in size and shape to the mature gemmae of bunt; but transparent. Some tubes had emptied themselves, and their globules were seen strewn about them in the field of the microscope.

Doubtless in repeating my work, no one will take this plant for that of bunt, any more than I have. Several others of these are seen, even in preparations made with sufficient precautions, especially one that might more easily be mistaken, and bears globules very similar in color and size to the mature gemmae of bunt. In observations followed with too little perseverance, they might lead one to think that this latter plant is capable of completely reproducing its seeds in water, and that they can mature there. But these globules are smoother than the gemmae of bunt. They never appear engine-turned and they mislead the observer who expects to see stalks and aigrettes come from them. They resolve themselves into a multitude of little grains which reproduce similar globules borne on pedicels that arise like pins from a very long and very branched stalk.

the bunt, will have germinated without it; and, although the wheat seedling, in coming up, should carry some gemmae to the air with it, it will have left them in its path in passing through the paper and the soil packed over it. If a small number came up, they would not suffice to produce infection; for it is probably true of the seeds of this little plant, as of all those that are excessively numerous, that many perish for one that achieves its end.

114. I further observed that very dry wheat, dusted with bunt that also is very dry, does not ordinarily become very bunted, unless this operation be so conducted as to sow much of the bunt with the wheat.

115. In order for the wheat to produce many bunted heads, it must be moistened before it is inoculated, or fresh bunt must be used on wheat that is still soft, or, for greater certainty, the wheat must be allowed to begin germinating in water in which some bunt of the current year has been suspended, and sprinkled with this suspension again at the time of sowing; then few plants escape infection: sad secret, indeed; but the means of obtaining the highest degree of infection are also the best suited to test the efficacy of preventives.

116. When wheat is thus infected, the bunt germinates some days later on the sprout to which it is attached, and that has brought it into the air. Thus the conditions attending it are the most favorable for producing the aigrettes or tufted stalks, which penetrate the sprout. This agrees perfectly with what I already have proved, that the introduction of the bunt plant into the wheat plant is possible only at the time of the germination of the latter, or very soon afterwards.

117. The results of the ingenious experiments made by M. Tessier, in order to discover whether certain parts of wheat seed are more susceptible to contagion than others, explain why it is necessary to moisten the grain before applying the bunt to it when one wishes to succeed more surely in infecting it.

"I dipped," he says, "the point of a pin into some of the moistened bunt dust, with which I impregnated only the upper part of the germ of 140 kernels of wheat, after a washing and liming capable of removing from it even the last vestige of bunt; 140 kernels of the same wheat were smutted in the groove, and

140 in the tuft. These different kernels were sown in three separate rows, at equal distances, in the open field, in the midst of a large number of other experiments. The 140 seeds inoculated on the germ produced 40 bunted heads; the 140 inoculated in the groove produced 21 of them; and the 140 inoculated on the tuft bore only 10." (*Art.* Carie, *already cited*, p. 704.)

M. Tessier later repeated this experiment on a larger number of seeds. The results were not strictly the same each time, probably because of the conditions; but each time he had many bunted heads; and, in general, the kernels inoculated on the germ gave more of them than those inoculated on the tuft or the groove.

It is clear that the germ of wheat, in developing, probably encounters more of the seeds of bunt the nearer they are to it. If they are only upon the little tuft at the other end of the seed, the plumule, in turning and curving around the seed, must go there to meet them. Now, although that happens quite often, because it is rare that the seed is placed with the germ above, it is conceivable nevertheless that the encounter of the germ and the seeds of the bunt should be much less frequent in this latter case than in the former. If, then, some very dry bunt powder is dusted on seed which is also very dry, only the tuft will retain it: the rest will slide off the seed either during sowing or covering. Rains often will wash off what manages to remain. On the other hand, if the seed or the bunt is moistened, the latter will stick on the entire surface of the former by means of the gluten with which it is provided, and it will be found on the germ in large enough quantity that the plants which will arise from it may penetrate in sufficient number.

118. M. Tessier regards the bunt dust as a virus, and he points out how great its activity is, since such a small quantity as he could take with the point of a pin, "applied to clean seeds, at certain points only, is capable of spoiling so many heads"; for, he adds, "in certain rows of inoculated seeds, often a third or a half of the heads were spoiled." But this very small quantity of bunt contained thousands of gemmae one of which alone, in favorable circumstances, might have been sufficient to poison all the heads of an individual wheat plant.

119. Our present knowledge of the cause of bunt explains so

naturally all the phenomena pertaining to it, and concerning which reasonable doubts cannot be raised, that this alone, assuming it to be only a matter of a simple hypothesis, would constitute a marked presumption in its favor. One will further be convinced of it by attempting to apply it in explanation of the numerous and important observations by which MM. Tillet, Duhamel, and Tessier have enriched science.

120. One only of the experiments of M. Tessier does not at first appear to be in accord with the well-ascertained fact that bunt is a plant of a kind approaching that of the uredos, the grain or the seeds of which form the black dust that fills the bunted kernels. This experiment follows:

"By chemical analysis of bunt, I had obtained," says M. Tessier, "different products, a thick oil or sort of butter and an extract. I attempted to inoculate with each of these products, as I had inoculated with the bunt dust, which had not undergone the action of fire.

"The inoculation with the thick oil produced as much as a quarter and even nearly a third of bunted heads; and that of the extract produced nearly as much as a third of them."

But it is very probable that part of the dust passed with the oil, or ahead of it, at the beginning of the chemical operation, without having been exposed to a heat severe enough to destroy in the gemmae the faculty of germination. Another part could remain in the same manner in the extract or gain access to it after the operation. It is conceivable that the air and all the utensils of a workshop where this dust was analyzed could serve as vehicles for it; and its effect was then the more assured, since a tenacious substance held it on the wheat.

There should, therefore, be nothing in this experiment that cannot easily be explained by the introduction into the wheat plant of a parasite that begins to germinate outside; and I venture to say that bunt will not be inoculated with oil that is entirely purified from the dust that the bunted kernels contain; that is to say, with such oil that, in examining it under the microscope, one sees none of the globules of which the dust is composed, and which are the gemmae or the spores of this growth.

121. Bunt suspended in boiling water very quickly loses its germinability. It loses it after a few hours in water at 50° R. and,

when it has remained for some time in water at 40°, it produces, everything else being equal, far fewer stalks than that which was not subjected to this degree of heat. It did not produce any of them at all, at least at a not very high temperature, when it had been exposed dry and spread out for four or five hours, at a temperature of 56° R. But such as sticks tightly, as it were, to the wheat that it contaminates can endure this test much longer without being so much altered by it, because of the moisture the grain furnishes it in proportion as its own evaporates.

"I tried," says M. Tessier, "treatments from 20° of heat up to 80°, and I assured myself that the diminution of bunt was not at all because of the degree of heat of the treatment, and that it was indifferent to heat at 20° or at 60°."

But bunt does not undergo, during liming, a temperature equal to that of the liquid in which the wheat is immersed, or with which it is sprinkled; that depends also on the temperature of the wheat and on its bulk, compared to that of the liquid.

Several times I sowed bunt-dusted wheat upon which, after having placed it in an earthenware vessel, I had thrown enough boiling water to wet it well. It did not come up so well as did wheat treated in the same way without having been scalded; but it always produced less bunt.

122. Very severe frosts hinder but very little, if at all, the growth of bunt, provided they do not occur too near the time when it begins to germinate.

123. The older the bunt, the longer the time required for its growth to begin, all other things being equal; and, when it is very old, it does not germinate at all. M. Tillet inoculated seed for 20 years with bunt dust of one and the same year; little by little it lost its activity until it no longer produced any effect. This agreement of results obtained by such different means is remarkable.

I dusted wheat with bunt six years old. It did not give so large a proportion of bunted heads as that inoculated with fresher bunt. About 15 months later, this same bunt was unable to germinate in water at a temperature of 10 or 11 degrees; it germinated when the temperature was raised; and six months later, that is to say, about eight years after having been collected, it could no longer germinate at all, however high the temperature. That of

M. Tillet was kept in a sound state for a longer time. This may depend on the climate and several other conditions, as happens with various seeds. I tested some five years old, which germinated 24 hours later than that of the current year. In some way, I hastened the effect of time on bunt by exposing it often to the heat of the summer sun in glasses covered by paper.

Bunt, detached, either by washing or by agitation while dry, from a little wheat that had been thus treated, could not germinate at a temperature that varied, while it was soaking, from 14 to 11° R.

124. Bunt, alternately moistened and dried, in the shade or in the sun, does not lose its germinability, provided it is not allowed to dry at the time when it begins to grow, or too near this time, and provided the temperature is not very high during the experiment.

125. Bunt, washed with the most scrupulous exactitude, nevertheless germinates about like that which has not been washed. It then loses its fetid odor, and assumes one resembling that of cinchona. It gives a rather pretty, yellowish green color to water in which it has remained for some time. The first waters are extremely fetid, especially when the bunt is fresh. Probably this water contains the altered gluten, whose analysis MM. Fourcroy and Vauquelin have given in a memoir, read at the Institute in the year 13. But I presume that the analysis of the spores of well-washed bunt would give different results from those these celebrated chemists obtained from the entire bunt.

126. In general, bunt does not germinate in water in which acids, alkalies, salts, alcohol, or camphor have been dissolved or mixed, even in very small proportions. But these substances do not always thereby destroy the vegetative faculty, and it often suffices to wash the bunt in pure water, for it later to grow as usual.

127. Of these substances, a few, among those sufficiently soluble in water, permanently destroy the faculty of germination in bunt, even when it remains in them only a short time and is later washed in pure water.

128. The numerous experiments performed by MM. Tillet and Tessier on bunt-contaminated wheat, and from which they have deduced some more or less preventive methods, are per-

fectly in accord with those whose general results I have just given, and that I have made directly on the germination of bunt; but the substances with which these scholars have terminated [their experiments] do not appear to me to combine all the qualities that constitute a convenient and economical preventive. We shall see how the discovery of the cause of bunt has led me to that of this preventive.

VIII

Some Preventives

129. Having proved, as we have seen in the preceding section, that an infinite number of substances when placed in water give it the property of destroying the germ of bunt spores, it remained for me to investigate which of these substances combine in the highest degree all the qualities of a preventive applicable to agriculture. I had begun to proceed methodically towards this, when a fortunate incident considerably shortened this work.

130. Having washed some bunt with several changes of well water, then with water that had been distilled in a large copper alembic, and having left it for some time in a glass goblet containing this water, I placed drops of the water from the goblet, containing several hundred gemmae or spores of bunt, in a watch glass half full of highly purified distilled water. To my great astonishment, these gemmae either did not germinate or germinated very poorly, whereas some others, in similar circumstances save for the copper, germinated as usual. I decided then to direct my researches first and principally towards copper and the copper salts.

131. Scarcely had I begun to apply myself to this, when a friend of mine, to whom I related what had happened, told me that, on an estate at Villemade near Montauban, where every year he sows about 20 hectoliters of wheat, steeped in milk of lime prepared in a large copper cauldron, he never has any bunt, while the neighbors who steep it in a wooden vessel, as is generally done thereabouts, very often have it, and sometimes in very large amount.

132. A little later, M. Lagravère, deputy mayor of Montauban, told me apropos of my observations, that for a long time he

had no bunt at all in one of his fields, where a large basket-shape copper vessel, pierced with many small holes, was used for steeping. Neither M. Lagravère nor the proprietor of Villemade suspected that the copper was of consequence in this effect, and they supposed that the liming was done with more care at their places than at their neighbors'.

I visited the Villemade estate at the approach of harvest time. I found, in fact, much less bunt there than in the neighboring fields, and the difference sometimes varied from a half to 1/150 of the number of heads. It was especially conspicuous in a small remote field enclosed by quite a large piece of land, although the two were separated only by the common hollow of the furrows; so that, in walking around the furrows that served as limits to the two fields, one found much more bunt on the side of the small one than on the other.

134. I was shown the copper vessel. It was encrusted within by a calcareous layer, which covered another one of verdigris. The latter could be seen in small places, which might form in all about a square decimeter, where the former had become detached. The effect probably was due entirely to this verdigris; but it did not prevent absolutely, for there were some parts of the harvest, rare, it is true, principally in the shade of the house and groups of large trees, in which bunt was found in the proportion of 1/50 or 1/40. Besides, the effect of this liming should vary each year, according to the extent to which the layer of verdigris was uncovered or encrusted; but, upon this year's crop, this effect is the less doubtful because, independently of the proofs I have just given, there exists another very striking one. Some maslin had been sown that had not been limed, and of which the wheat produced a large quantity of bunt.

135. I later requested M. Lagravère to inform me in detail concerning the manner in which he did his liming.

The copper basket is filled with wheat; it is immersed in milk of lime mixed with a little sheep's urine; it is then placed upon some cross pieces above the tub containing the liquid, in order to let it drain, etc.

136. In observing this basket, which was smaller and still more encrusted with carbonate of lime than the Villemade cauldron, I suspected that its effects would be inferior, and I was

very soon convinced of it. On the whole, however, M. Lagravère had little bunt, and not nearly so much as his nearest neighbors.

137. The most of it occurred, as at Villemade, in the shade of large trees and of habitations. Several other observations and the information I procured convinced me that shade favors the propagation of bunt, although it can not give rise to infection when the spores of the internal plant are not present.

Shade can produce this effect by inhibiting the growth of the wheat plant. The more vigorously the wheat grows, all other things being equal, the less it is subject to bunt; and, reciprocally, the bunted plants are generally less vigorous, or shorter, than the others. This has been observed earlier by others, but I saw it this year (1807) in a very striking manner. In 1806 I inoculated 56 decagrams of wheat. I washed a part of it, which I sowed in November. At the same time, I sowed the rest, taking the necessary precautions in order that there be no confusion. From the middle of April, a great difference in the height of the plants of the two parts was noticeable. However, the part that had been washed was not free from bunt: this disease had attacked 1/50 of its heads and six-sevenths of the others.

Shade further favors the propagation of bunt by maintaining a certain degree of moisture; and this is not at all contradictory to what MM. Tillet and Tessier have proved, that fogs are not the cause of bunt; for, without being the cause of it, they can favor it.[17]

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[17] M. Tessier regarded fogs as the cause of rust; but it is proved today that they are no more the cause of rust than of bunt. How could fogs be the cause of a plant? They are able at most to favor its propagation, which, nevertheless, is perhaps not yet proved. In March, I sowed wheat that became more rusted than some of the same wheat sowed in autumn, and much less so than some of the same sowed in April. The rust seems then to be in inverse relation to the total duration of the fogs to which the wheat was exposed. Some observers have maintained that the rust appears all at once; that in an instant everything that grows in a vast country is affected by it. This is an error. At least, it is certain that it does not happen thus everywhere. The rust of the leaves of wheat, the linear uredo of de Candolle, appears little by little, and ordinarily very slowly. Sometimes the lower leaves of the same plant are laden with it, while none of it is yet to be seen on the leaves above. At other times certain plants bear it, while in the same field other plants do not. It often happens also that wheat is much rusted and that other species of plants do not show any rust at all. Moreover, the rust that attacks one plant is rarely the same species as that attacking another; and, when it begins to appear externally, it is found in the interior of the leaf if one searches carefully.

Effects of Copper on the Germination and Development of Bunt

138. Ten or 12 milligrams of fine copper dust prevents bunt from germinating, when mixed with some of it and suspended in 15 decagrams of water. A sheet of yellow or red copper of 15 square centimeters, scraped or not, either from base coin or from bell metal, produces the same effect.[18]

In order to attempt to determine the limits of the influence of copper on the germination and development of bunt, I used a thoroughly scraped sheet of this metal, the total surface of which was 15 square centimeters (2 inches).

I let it soak for a longer or shorter time at the bottom of a tumbler in three decagrams (one ounce) of ordinary water. I removed it, more or less oxidized, black, brown, or iridescent. I next made a suspension of bunt in this water and left it there until the bunt germinated or the time for its germination had long passed.

140. This sheet must remain 60 or 72 hours in the water, in order that the latter, at a temperature of five or six degrees, produce a well-marked effect upon the growth of bunt. Then it either does not germinate at all or germinates very poorly and produces only deformed stalks. These stalks are found only at the surface of the water, and no sign of growth is found at the bottom.

141. Similar effects are obtained from the prolonged stay of water in very clean copper vessels; but, when it is shaken often, especially if there is a little verdigris, or carbonate of copper, etc., on the surface of the copper, the effects are much more marked.

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[18] If, as it seems, the ancient Greeks or the ancient Romans were not acquainted with bunt, it probably was due to the brass they frequently used in their farming implements.

Dalechamp says, in his *Histoire générale des plantes*, that Theophrastus and Pliny spoke of *blight*, or *smut*, under the names of ερυσιδχ and *carbunculatio;* but neither these authors nor Dalechamp makes mention of *bunt*, which accords with what Ginani says, that this disease was entirely unknown in all Lombardy before the year 1730.

Would all the time that elapsed from the time when they no longer made any great use of brass in Italy up to this period have been necessary, in order for this disease to recur? But lands impregnated with copper for long ages may be protected for a long time. However this may be, its recurrence should not be attributed, as Ginani thinks, to a change in climate.

142. The extent of the copper surface moistened by the water cannot take the place of time, unless it be very great; for, when it was increased nearly tenfold, and when the water was left in it only 20 hours, the latter did not perceptibly inhibit the germination of bunt held in suspension in it.

143. Applied to agriculture, these procedures would be subject to several disadvantages. They would require much time and could be of use only for small quantities of seed. I next undertook to determine carefully the effects of various copper salts on the germination of bunt; but copper sulphate (commercial blue vitriol), being both easier to procure and subject to fewer disadvantages than the others, particularly interested me.

Some Effects of Copper Sulphate on the Germination of Bunt

144. I had noted that it was sufficient for the water in which the bunt had been suspended to hold in solution a very small proportion of copper sulphate, in order to prevent it from germinating; but I wished to know to what point this proportion could be diminished without losing the effect. I then performed experiments with smaller and smaller quantities at a temperature of 5 or 6°, at which bunt consequently takes a long time to germinate. I found that the constant presence of this salt in the proportion of 1/280,000 of the weight of the water prevents all germination; that 1/600,000, or even 1/1,000,000, retards it perceptibly; that a solution of 1/10,000 by weight of this sulphate in water suffices to destroy the viability of bunt soaked in it for one or two hours, even though it be washed immediately afterwards; and that, in experiments of the kind last mentioned, much less time or less sulphate retards germination.

145. According to the experiments of M. Proust, copper sulphate, such as I used, contains 35 per cent of water of crystallization. Thus, the real sulphate necessary to give water the faculty of preventing bunt from germinating in it at a low temperature is less than 1/400,000 by weight, and 1/1,200,000 retards its germination.

146. In repeating these experiments at higher temperatures, I sometimes found that bunt germinates in water containing 1/200,000 of its weight of copper sulphate, and that 1/10,000

does not destroy its viability, when it is washed, after having been allowed to soak only a few hours in the solution.

147. These differences are not surprising. At a high temperature, the more active viability either does not give the preventive time to act or more strongly resists its action.

148. But, as was more difficult to explain, at a very high temperature things sometimes happen as they do when the temperature is much lower.

149. I suspected that this might arise from the dryness or moistness of the bunt. If it is very moist when placed in the solution, it is clear that, being already soaked with water, it absorbs less of it than when dry.

150. In order to assure myself of this, I took some very fresh bunt that I had just collected and exposed part of it for a few hours to a very hot sun in a window with a southern exposure, where the decimal thermometer rose to 56°. In three glasses, A, B, C, I made suspensions of the bunt thus dried, the first with pure water and the second with the preventive at the *minimum.* I soaked the third for three hours in a solution containing 1/10,000 of its weight of copper sulphate and then washed it. At the same time, I prepared three other glasses, A′, B′, C′, respectively, like the first, with some of the same bunt, which had not been dried. These latter suspensions, as well as A, grew about as usual; B and C produced nothing.

151. It should be noted that when the preventive does not act so well as usual under conditions that at first glance appear identical, the growth is manifest only at the surface of the water, and none or scarcely any of it is found at the bottom; whereas in pure water the gemmae that remain at the bottom, and are much more numerous than those at the surface, nearly all put out long bare stalks, when they are not stuck together in too large numbers.

152. When the sulphate is dissolved in ordinary water,[19] as I nearly always do in these experiments, in order that they may be more applicable to practice, there forms a bluish or greenish-white precipitate, which remains suspended in the liquid a very

[19] The ordinary water I use is very clear and good to drink and contains scarcely 1/2,800 of its weight of foreign substances in solution. These are principally calcareous salts and muriate of soda.

long time, and probably is a mixture of sulphate of lime and carbonate of copper. The copper sulphate then is decomposed in greater proportion as its quantity in relation to that of the water is decreased; so that, if a very weak solution is filtered, the liquid that passes through does not produce nearly so great an effect.

153. Does the precipitate act upon the bunt; or, indeed, could the filter act upon the sulphate? However it be, the precipitate and the solution in which it is suspended act, together or separately, upon all the bunt that the solution is capable of completely wetting.

154. With distilled water I tried 1/200,000 of its weight of sulphate; but at a high temperature it produced almost no effect. With 1/100,000, on the contrary, bunt gave absolutely no sign of growth. With 1/8,000, letting it remain in the solution only a few hours and then washing it, it grew almost as in pure water. Treated in the same manner for only a half hour in 1/2,400 at a temperature of 11° R., it was unable to grow.

155. These experiments have been repeated, for the most part, many times, especially when their results presented some uncertainty. They require more time and care than one would at first imagine; although, in general, they yield within a few days results analogous to those that would require eight or ten months in the field.

156. I am giving here only the summary of the experiments on bunt that I have tried in order to determine the limits of the action of copper sulphate and the manner in which it acts when dissolved in small quantity in ordinary water, the lengthy details seeming to me to be unnecessary.

The first point needs little more attention; the second would still require many experiments, to which I propose to give my attention.

Test of Copper Sulphate and of Copper on Wheat Dusted and Sown in the Open Field

157. Towards the middle of March of this year (1807), before having made the majority of the experiments, whose results I have just given, and as soon as I had made enough of them on copper and the copper salts in relation to the germination of bunt to discover their efficacy as preventives, I prepared several

samples of wheat, and sowed them in the field, although it was a little late.

I had (1) left some well water for 48 hours in a large pot made of red copper, which was slightly oxidized or tarnished by a little verdigris, stirring it at times with a stick. Into this water I put some wheat dusted according to the first procedure described in §115. I later took it out and let it drain, and then dry on paper in mild sunlight. (A)

(2) I filled the same pot up to the height of five or six fingers with boiling water. When this had cooled again, I immersed in it some bunt-dusted wheat, which I treated like the preceding. (B)

(3) I dissolved six decigrams (12 grains) of copper sulphate in a quantity of water sufficient to moisten well 15 hectograms of infected wheat. The wheat was worked up in this water with the hands; and when it had soaked it up, it was spread out upon paper, on which it was dried like the others. (C)

(4) I did the same thing with 36 grains of sulphate, instead of 12. (D)

These samples were sown with some others in the order in which they are shown, with the results, in the table of §162.

158. Having washed some seed from each of these samples of wheat, and having placed the washings in glasses full of water, I found that the bunt gemmae or spores that had been detached from A germinated only very tardily, and produced but a few very short, deformed stalks, from which no growth ensued; that the gemmae from B germinated as usual; and that those of C and D did not germinate at all. From this I had to conclude *a priori* that A, C, and D would yield no bunt, and that B would yield as much of it as if it had not been prepared after having been dusted, or that it would yield as much as if, after having been dusted, it had been passed quickly through some pure water.

159. It is not easy in this country to have things done as one wishes by peasants: their awkwardness or their unwillingness nearly always occasions some derangement in the experiments.

Various circumstances obliged me to have this wheat covered by the plough, and prevented me from supervising the operation to its finish. The result was that parts A and B, which were

162.

Wheat sown without treatment. Inoculated wheat, sown with or without treatment.

Soaked in water that had been left two days in a copper pot, in which it was stirred from time to time, rubbing a little upon the copper: one-sixth bunted.

Soaked in water that had boiled in the same pot, after this water had been cooled: one-sixth bunted.

Well moistened in a solution of copper sulphate containing this salt in the proportion of four decagrams to each hectoliter of wheat: 30 bunted heads among about 3,000.

Id. 12 decagrams of sulphate per hectoliter: 9 bunted heads among about 3,600.

Limed: 1/50 bunted.

Inoculated, not treated: one-third bunted.

About 1/150 bunted.

Note. *According to observations made on the bunt detached from B in washing it after treatment, it should have given a great deal of bunt. A, on the contrary, should not have given any at all if the two samples had not been mixed in covering them with the plough. The little bunt there was in C and D surely arose from the same cause.*

separated by only a small space, because I did not at first regard the difference between their anticipated results as very important, were entirely confused, especially because they were placed at an end of the field where various causes contributed to this effect.

160. A, according to the experiments performed on the washings, should have been much less bunted than B, or rather should not have been bunted at all.

But the results of these two trials are scarcely of practical significance, because copper thus employed has some disadvantages, as we have seen, and it is not necessary to have recourse to it.

161. If, on the other hand, one compares A, B, E, and especially F, with C and D, the effect of the sulphate will not seem doubtful. The nine heads of D are an insignificant quantity, and arise without doubt from F or E. It is conceivable that the plough-share or the feet of two oxen and a man, which pass and repass several times upon the same furrow, could well transport a few seeds from one part to the other, in spite of the care taken to leave quite wide spaces between them.

163. After this wheat was sown, experience having taught me that the efficacy of copper sulphate was much greater than I had at first supposed, I attempted some other tests in the field with smaller doses. They were sown and very carefully covered. Nothing should be mixed this time. But the season, already too advanced at the time of the first tests, was then still more so: it was in April. Only very little rain fell afterward, and this wheat gave too few heads to warrant any inference relative to practice. Nevertheless, if one wished to attach significance to these results, it would follow that two decagrams of copper sulphate, instead of 12, would suffice to protect completely each hectoliter of wheat.

In 1789, M. Tessier sowed some wheat that had been washed in a solution of copper sulphate, and found that it produced no bunt. However, this experiment could not prove anything, since it was made with wheat that, sown without treatment, yielded a crop only 1/1,800 bunted (Art. *Carie,* p. 721), a quantity that, in experiments of this kind, should be regarded as of absolutely no importance. It appears, moreover, that the solution was very strong, since the wheat, when dry, was spotted with blue, and the preventive would have been very expensive. Thus, M. Tes-

sier was not impressed by it. In fact, he had no reason to be, since copper sulphate, in this experiment, gave no more promise than several other substances that he tried at the same time. M. Tessier then discarded this preventive with several others, such as pure lemon juice, ether, mint brandy, pure brandy, oil of hartshorn, etc., which he had tried only out of curiosity.

Effects of Copper Acetate and of Some Other Metallic Salts or Oxides on the Germination of Bunt

164. It appears, according to the few experiments I performed on crystalline copper acetate, that it acts on bunt nearly like the sulphate. I did not devote much attention to it; but I tried, as being more applicable to practice, some experiments on the effects of common vinegar in which copper had been left.

165. I first found that the quantity of copper that the vinegar I used[20] could dissolve in a few days at a temperature of 20 to 22° amounted to 1/3,000 of its weight.

166. Three grams of this vinegar, holding in solution 1/3,000 of its weight of copper, mixed with 27 grams of water, destroys the germinability of bunt suspended in it for four hours at a temperature of 18 to 20°. Now three grams of this vinegar contain a milligram of copper, which, according to the experiments of M. Proust, gives nearly three milligrams of acetate, that is to say, 1/10,000 of the weight of the liquid. But, when the bunt is left in it only two hours, or when only four decigrams of the same coppered vinegar are mixed with 27 grams of water, the germination is only retarded, and some bare stalks are found at the bottom of the water.

167. I repeated the same experiments with vinegar weakened in different degrees, and always obtained results proportional to the copper it had dissolved; but, the weaker the vinegar, the more slowly does the solution take place.

168. Some experiments performed on sulphuric acid and on vinegar seem to prove, concurrently with those I made on copper, and I have already reported, that the effects produced by copper sulphate and acetate on bunt are due principally to copper, or rather to copper oxide, since iron sulphate does not con-

[20] In order to saturate 45 grams of this vinegar, it was necessary to use one gram of chalk called whiting.

stantly produce like effects, unless it be employed in much larger proportion.

169. It appears from the experiments I performed directly on bunt with white oxide of arsenic, which is used in York County, England, that it can produce a quite marked effect as a preventive. But its energy is much inferior to that of copper sulphate. Iron sulphate, to judge by a small number of experiments, would produce as much effect, and in this case would be much preferable. It costs less by half, and dissolves promptly and in large proportion in cold water, which dissolves arsenic only in small quantity. It is necessary to reduce the arsenic to a very fine powder and boil it a long time (at least such is the English practice). This does not fail to complicate the process greatly and to multiply the chances of the dangers that are encountered in using this terrible poison, which, moreover, it would be very imprudent to place in everyone's hands.

170. Superoxygenated muriate of mercury (corrosive sublimate) is highly potent; but it is too expensive and dangerous. M. Tessier, who obtained some success with these substances, formally advises avoiding them and reports some instances of grave accidents they have occasioned. They, nevertheless, prove that anything that destroys the germination of bunt is a preventive of infection.

The Method of Using Copper Sulphate on a Large Scale

171. Having treated about 50 hectoliters of seed with copper sulphate this fall (1807), I am going to describe the method that seemed to me the surest and most convenient.

One places in a tub as many times 14 liters of water as there are hectoliters of wheat to be treated, and dissolves in it as many times nine decagrams of copper sulphate.[21] There are two other

[21] This amounts to nearly 23 pints of water and four and one-half ounces by light-ounce weight of copper sulphate for each 12 bushels of wheat, Parisian measure.

The solution of the sulphate requires a few hours. It may be hastened by crushing it, by stirring, or by dissolving it in a small quantity of boiling water.

When the operation must be of long duration, it is better to prepare solution for only a part of the seed, and to add water and copper sulphate to it in the prescribed proportions. In order to give the salt time to dissolve, this can be done during intervals of rest.

After some of this solution, in which much seed had been treated, had been

vessels, each with a capactiy of two or three hectoliters. Twelve or 14 decaliters of wheat to be treated are thrown into one, and some of the solution is poured into it until it rises to several decimeters (5 or 6 inches) above the wheat. It is stirred well, and all floatage is carefully removed. Some wheat is placed in the second vessel, and treated in the same way. Some cross-pieces are laid upon this second vessel, and on them is placed a basket of such texture that it lets the water pass freely, without letting the grain pass. When the grain in the first vessel has remained under water for half an hour, it is dipped out with a *copper hand-bowl*,[22] a certain quantity of the liquid being taken at the same time. This is briskly thrown back, in order to free the wheat from any floating material that may remain. This wheat is then thrown into the basket; and, when the basket is full and the wheat has drained sufficiently, the latter is placed in a heap. When scarcely any more wheat remains in the first vessel, more is put in, stirred, skimmed, etc. The basket is placed upon this vessel and the manipulation is from the second to the first, as it was from the first to the second, etc.

Wheat thus treated is soon dry enough to be sown. If, however, it were necessary to keep it a long time, it would be well to stir it occasionally.[23]

172. Anyone can vary this process in his own way, according to the hands and utensils at his disposal. It suffices to proceed so as to attain the same ends, that is to say, the wheat must stay in the solution a half-hour, or more if desired, for there is no harm in this, and no bunted kernels must be left.

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kept for eight or ten days, a thick bed of mould was found in it, and the solution took on a green color. Evaporated after having been filtered, it furnished no crystals of copper sulphate, but yielded other small greenish-white crystals, mixed with a dull green substance.

In spite of this alteration, a quart of the solution, mixed with three quarts of water, destroyed the germinability of the bunt of the current year, which I left in it for 25 minutes.

[22] Deep and long-handle copper bucket.

[23] Fowls can eat it without injury. With it alone, I amply nourished a four-month-old chicken for six days. On the last day, I even fed it wheat freshly taken, after an hour's soaking, from the solution used for its treatment. This chicken kept quite well on this food; and, although fowls and pigeons rush avidly upon fields that are being sown, it has not been observed that this has injured them.

173. It is understood that one must avoid contaminating anew the treated wheat, either with the dust that escapes from severely bunted wheat in motion, or by transporting it in bags soiled by this dust.

174. However contaminated the seed may be, the treatment we have just described will destroy the germ of the bunt; but, since whole bunted kernels are not easily penetrated by the solution, there would be danger that some of them would still be left after the operation, if the wheat that is to be treated should contain a large quantity. In this case, then, it is essential to free the wheat from them by one of the most convenient methods.

175. Nevertheless, in doubling the dose of sulphate[24] and the time of immersion, and stirring and skimming as I have prescribed, I have every reason to believe that there would be nothing to fear from their influence.

176. It must be noted further, that the effect of the solution is more certain, the drier the seed is when immersed in it. If it were very moist, there would be danger that effectiveness would not result, or that it would not be complete. It would not be advisable, for example, to wash wheat beforehand to free it from bunted kernels unless it were later well dried.

177. If one worked with only a small quantity of wheat, it would be necessary for it to contain no bunted kernels; because, without increasing the dose of the solution or using a vessel very deep compared with its diameter, it would perhaps not cover the wheat sufficiently to permit skimming it. But there should then be no harm in leaving it there five or six hours, stirring it well from time to time. This should destroy the germ of bunt to the center of the bunted kernels. But, in any case, when it is not a

[24] There is nothing to fear from even a much stronger dose. I had to put in 16 times more of it than I indicate in the process, and to let the wheat soak 50 times longer in the solution, for it to be decidedly injured. By all that precedes, it has been seen that the bunt dust is composed of small spores, and that these behave quite like the seeds of the phanerogams. Now, M. Théodore de Saussure has demonstrated that, in general, salts are adverse to germination. Thus, it is not surprising that by dint of increasing the proportion of the sulphate, or the time of immersion, one succeeds in acting on the germ of the wheat; and, if there is nothing to fear for this germ from much smaller doses of sulphate, which nevertheless act so powerfully on that of bunt, this probably arises from the immense disproportion of the volume of the seeds, rather than from difference in their nature.

question of experimentation, it is better to use the cleanest seed obtainable. Nevertheless, I repeat, however contaminated the seed, the preventive will produce its effect.

178. In York County, England, as we have said (§169), arsenic is employed against bunt. They use only five grams for 14 hectograms of liquid, or four decagrams for each hectoliter of wheat. It will be objected, without doubt, that having said that the efficacy of arsenic against the germination of bunt is much inferior to that of copper sulphate, I nevertheless recommend a larger proportion of the latter.

But, (1) is it indeed certain that arsenic, employed as it is in York County, produces all of the effect that may be desired;[25] and if it is not employed in a stronger dose, is not this because the water can not hold more of it in solution in the process used, rather than because this dose suffices?

(2) Is not the effect obtained due in part to the large amount of stable urine that is added to the solution of arsenic?

(3) According to the experiments I performed on the germination of bunt, it is extremely probable that a quantity of copper sulphate, a great deal less than that indicated for the arsenic, would be sufficient; but I established the dosage that I recommend using upon the results of experiments I tried in the field.

[25] I suspended some bunt in water that held in solution the same proportion of arsenic as the liquid in which the wheat is immersed in England to treat it; and I let it soak there for one hour and 10 minutes (A), for two hours and 20 minutes (B), and for four hours and 20 minutes (C), and then washed it.

Two and a half days after the germination should have begun, C had as yet produced nothing. Four days after this time, several aigrettes and some other stalks were at the surface. But the growth of A and B had only been retarded several days; and, moreover, had taken place, as usual, at the surface of the water.

At the same time I suspended some bunt in a solution of copper sulphate at the dosage I advise using, and left it here only 20 minutes before washing it. This sufficed to prevent it from germinating.

I have since proved that five minutes suffice for the preventive I recommend to produce more effect upon the germination of bunt than arsenic for four or five hours at the dosage prescribed by the English. We have seen that a solution of copper sulphate in water that contains only 1/10,000 of it by weight often suffices to destroy the germinability of bunt soaked in it for a half hour. I have proved that, in the same circumstances, four hours do not always suffice to produce the same effect in a solution containing 1/1,200 of its weight of arsenic. Thus, in regard to the time necessary for these two substances to act, as in the case of the intensity, the action of the copper sulphate is incomparably greater than that of the arsenic.

Moreover, although it is advisable, for this year at least, to adhere to this dosage, in order to be sure of success, I can scarcely doubt what would result from much smaller dosages, such as three decagrams, two decagrams, and even a single decagram, to each hectoliter of wheat. I tried this on several hectoliters of seed this year. The economy, after all, could never be very great, since, when the sulphate is bought at retail, it costs only 25 centimes, or 5 sous, per hectoliter.

179. Besides, there might be some particular cases in which it is possible that the preventive would not succeed; as, for example, if at the time of seeding, or shortly before or after, the wind brought upon the soil a large quantity of bunt from some neighboring place where wheat was being cleaned on a large scale. That, however, is extremely rare.

M. Tessier reports "... that, at the end of June, 1786, a farmer hauled into a piece of land that was to be sown in wheat in the following October, three cartloads of dust and débris from his barn, after having threshed his wheat, in which there were a great many bunted heads. This trash was left in three heaps for several months. At the time of the last working of the field, it was spread in the usual way, so as to leave as little of it as possible at the place of each heap. The remainder of the field was treated with ordinary manure. In 1787 half of the heads were bunted in the places where the trash from the barn had been left for a long time, a quarter in those upon which it had been spread, and none or very few in the rest of the piece of land. The same wheat and a steeping in lime and fowl's droppings (to seed the whole field) had been employed. The space manured by the trash occupied 15 to 18 rods."

The sulphate probably would not have protected these 15 or 18 rods; for, in whatever way bunt or bunted kernels come to be found abundantly upon the land, either when it is sown or some time after, it is not probable that the sulphate can protect, unless, perhaps, it is used in a very strong dosage.

180. Here the bunt and the bunted kernels were preserved intact in the three heaps. If it had rained much after the month of June, especially if, according to this supposition, the trash had been spread early, it is probable that all the bunt would have germinated too soon to be able to infect the wheat or that which

was contained in the bunted kernels would have become incapable of germination, as happened to that of the bunted kernels I kept for a long time under water. These facts agree perfectly with all that I have previously said concerning the cause of bunt and with what I conjecture concerning the final cause of the puccinias of the uredos, which are very probably retarded seeds, seeds that should not germinate until a long time after they have become detached from the plant on which they have grown. These facts likewise agree perfectly with what I have proved concerning the bunted kernels, which serve as retarded seeds of bunt. It is known, furthermore, that these delayed seeds are not without parallel in the phanerogams.

The Method of Using Copper Acetate on a Large Scale

181. Some vinegar is placed in a copper vessel, care being taken not to fill it, and left there 20 or 24 hours. It is then withdrawn and replaced with fresh vinegar in sufficient quantity to dissolve the verdigris, which is formed on the parts of the vessel adjacent to the liquid, and has not been wet by it. This solution is aided by rubbing with something appropriate. This new vinegar is added to the first, in order to make use of it, as will be explained.

But let us note (1) that the vessel should be clean, unless it be soiled by verdigris, or copper oxide or carbonate, which would then be dissolved by increasing the quantity of the vinegar;

(2) That, if the vessel is of large capacity, the vinegar should be put in only up to a limited height; otherwise, it would have to be left there longer, in order to enable it to acquire the desired property;

(3) That it is advantageous to cover the vessel while the vinegar remains in it, and to stir it from time to time, although this precaution is not indispensable;

(4) A part of this solution is mixed in four, five or six parts of water, according to the strength of the vinegar that has been employed, and it is used to moisten the wheat, as has been prescribed for the sulphate. But I forewarn that this process has not been proved upon wheat and that copper sulphate, a specific of which I have assured myself by a much greater number of experiments, is much more valuable, is generally cheaper, and is not

subject to any disadvantages. I propose the acetate only for those who might not be in position to procure the sulphate when needed.

Lacking large copper vessels, one may put into wooden vessels, with the vinegar, the débris of copper utensils, or copper coins, which will not undergo perceptible change (§165).

Limings

182. M. Tessier, after having tested a large number of different substances as preventives of bunt, proposes four methods in which lime is the principal ingredient. According to him, this substance is the only one that acts efficaciously: "Without lime (p. 712), none of the recipes that have been proposed against bunt would have well-marked effect: it is the lime that gives them this degree of activity so necessary to purify the seed from the contagious dust. Thus, it is always recommended to pass the seed through lime, in whatever manner it may be advised to treat it otherwise." He seems to compare its efficacy relative to bunt to that of cinchona against the intermittent fevers. "However, it sometimes happens that with the best recipes one does not succeed, and it is probably because the lime is not good[26] or because it is not used properly and not reinforced by indispensable care. Moreover, when the wheat in the sheaf is full of bunted heads, or when the threshed grain is suspected of being or is perceptibly contaminated by bunt, much is risked by being content with liming it. I have sometimes been fortunate enough to harvest no bunt at all from fields, the blackened and contaminated seed of which had been subjected only to a most careful, long, and perfect liming. But I have not succeeded completely every year; it is surest then to precede the liming by one of the following treatments:"

183. These treatments are of six kinds:

(1) Cleansing by hand selection. This is the simplest. In 1790, within a radius of 45 to 50 miles of Paris, when the bunted heads were not too numerous, and by having this work done by women, it cost about 12 sous per hectoliter of seed.

(2) Cleansing by threshing the bunted stems upon a cask or a cylinder.

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[26] It has recently been found that lime cooked with wood contains much potash, which should render it more active in liming than that cooked with coal.

(3) Cleansing by threshing with a flail with powdered earth.

(4) Cleansing by milling.

(5) Cleansing by sifting.

"Nothing is more expeditious for sifting," says M. Tessier, "than small wire. Disposed in an inclined plane, and formed of iron wires pressed taut against one another, it receives the grain from above, and the latter reaches the bottom only after having been shaken and battered about. By repeating this operation several times, the black color may be entirely removed.

"In 1782, in order to learn what degree of cleansing could be obtained by this means, I sowed some wheat that had been very much contaminated with bunt, but had been passed 12 times over an iron wire sieve, and some of the same wheat that had been passed 30 times over this sieve. The latter was so thoroughly cleaned that there was no longer occasion to hope that further sifting would clean it more; it could be shown to advantage in the markets. The first yielded 25 bunted heads per sheaf, and the second only 11,[27] that is to say, less than half as much. In the same year, unsifted wheat produced 1/4 of bunted heads. It is then proved that siftings, more or less often repeated, purify the wheat of more or less of the bunt, without wholly removing it."

(6) Cleansing by washing in water.

"It is not necessary," says M. Tessier, "to use very cold water, which would shrivel the grain, instead of cleaning it. Warm water succeeds much better. The washings do not remove all sources of bunt. I have sown wheat after having washed it in three and in six waters. The first produced one-fifth, and the other one-eighth of bunted heads. The same wheat, sown without being washed, yielded more than one-fourth of bunted heads." (*V.* §137.)

M. Tessier cites a method of M. Girot, which seemed to him exact and sure (p. 716).

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[27] The bunted heads often stand very low, as M. Tessier has observed; so that a large number escape the sickle and remain in the stubble. It is not possible, therefore, to judge the true proportion of healthy and bunted heads by the grain in the sheaf. But this experiment proves, nonetheless, that the more the wheat is passed over the sieve, the less it is contaminated. I mean only that it must not be concluded from it that the number of bunted heads in the field in the first case was only 25 per sheaf, or only 11 in the second:

184. He is convinced that, without these preliminary operations, liming does not succeed. "Several times," he says, "I have succeeded in harvesting wheat free from bunt by employing only simple liming; but this was in years in which the seed wheat was only slightly contaminated. In 1785, I divided a field into nine parts. Wheat almost entirely black with bunt was sown here, after eight portions of the seed had been steeped in different doses of lime. These lots of seed produced a great quantity of bunted heads, some one-fifth or one-fourth, others one-third or even more. At the harvest of 1786, the wheat that had been subjected to no treatment, did not yield more bunt. I had placed the seed of the ninth part in a bucket full of lime water, in which it soaked for 24 hours. I found in its yield only 1/25 of the heads bunted.

185. "It follows from this experiment, (1) that liming alone cannot protect from bunt the yield of wheat from seed highly contaminated with it; (2) that when it is contaminated to a certain degree much care is necessary to put it in a state of adequate purity; (3) that, if absolutely necessary, by letting it soak a long time in lime water, the contagious principle would be almost entirely killed. But it would be necessary to skim off the bunted kernels, which, without this attention, would continually be crushed in the manipulations of liming, and would give the wheat the disease from which it is sought to free it; and, yet, there is no assurance that some of these would not escape the most scrupulous search."

186. Let us note here that, when I advise removing the bunted kernels carefully when the wheat is passed into the copper sulphate, this is not for fear of their being crushed in the solution. On the contrary, I am quite sure that, if it remains there for some time, the dust of all those that are crushed will lose its viability, and consequently, no longer be contagious. I fear only that the intact bunted kernels may later be crushed in the treated wheat, either infecting it anew or being scattered upon the field with the seed. There would be nothing to fear even from these bunted kernels if they had soaked five or six hours in the solution, or only one hour in a solution twice as strong. They would then be sufficiently penetrated by it for the gemmae they contain to be incapable of germination.

187. M. Tessier has noted "that a large number of landowners and farmers protect themselves from bunt by using only lime and water in a large dosage." "As for myself," he adds, "I have employed this method only in large-scale cultures and in years when the contagion of the bunt was very widespread, and harvested scarcely any bunt." It should be understood, to be sure, that the seed had been cleansed before being limed. Otherwise, this would not seem to accord with either what precedes or what follows: "When in some comparative experiments," he continues, "I used more or less attenuated dosages of lime, I had more or less bunt. . . . But in this case the wheat had been *well sifted and washed* before being limed; for if various proportions of lime were tried upon seed that was much contaminated with bunt, without a previous cleansing, the seed treated with a strong dose of lime would produce as many bunted heads as that treated with a weaker one. I have at least tried it by weakening the proportions up to one-eighth. The reason for this is simple. It is that when the seed is so contaminated that it is black, whatever the quantity of lime employed, it removes only a part of the bunt, and much always remains. Not all of the dust is active in the production of bunt but only a small portion of it. So long as this portion remains, one should expect to harvest many bunted heads. Now, in the circumstances in question, lime and any other substance that may be added to it, exerting their action upon the greater part of the *bunt*, would not attack the dust that adheres closely to the wheat kernels, and its full effect would take place, which proves further the necessity of a cleansing."

188. It seems to me that this explanation is satisfactory only in so far as one supposes that the action of the lime on the bunt is purely mechanical; that it forms upon the seed a crust that, in becoming detached, carries with it a part of the bunt, an effect that may take place up to a certain point when it is used in large dosage. But, although the lime has only a quite weak chemical action upon the germination of bunt, it has one, nevertheless; and, in general, bunt does not germinate at all, or germinates very poorly, in filtered lime water. But, if it is allowed to soak in it for a few hours and then washed, it germinates about as usual. If it is soaked a long time, its germination is retarded or diminished, and a greater or less part of the gemmae lose their

germinability. It would seem, therefore, that, in addition to its mechanical action in removing bunt, lime must also destroy the germinability of a certain portion of the gemmae that adhere to the kernels.

189. But if the lime acts only mechanically on bunt, or if its chemical action on the germination of this seed is so weak that it is able to produce a very marked effect only under certain conditions and after a long time, this is not true of several other substances and particularly of copper sulphate. It suffices for bunt to soak a few minutes in a solution containing only 1/100 of its weight of this salt to lose its germinability irrevocably; but, in order that this effect take place in such a short time, the bunt must disseminate or become suspended. The gemmae compressed within the bunted kernels, being unable to dilate promptly enough to take up the solution, do not become disorganized, unless these bunted kernels soak for a longer time, or unless the solution is stronger. However, this is not so true for the gemmae that are at the surface of the wheat as it is for those that are enclosed and compressed within the bunted kernels. As soon as the wheat seed is well moistened, these gemmae are, necessarily, also wet; and, thereafter they soon lose their ability to germinate or propagate bunt.

190. In the first of the four methods of M. Tessier, the lime is used only with water;

In the second it is used with some neutral salts;

In the third with volatile alkali;

In the fourth with fixed alkali.

In the first three methods 20 pints of water are used per hectoliter of wheat,[28] and in the fourth, 25 pints.

In 1790, in the vicinity of Paris, the ingredients of the first method cost 25 centimes, or 5 sous, per hectoliter; those of the second and the third, 20 centimes, or 4 sous; those of the fourth, 70 centimes, or 14 sous.[29] This does not include the price of the

[28] M. Tessier says, p. 725, that to wash 24 bushels [two setiers, Parisian measure] of wheat in lye according to this method, one bushel of pigeon droppings, etc., must be placed in 260 pints of water; and, on page 729, he indicates by the same method only 3,250 pints of water for 1,200 bushels. He must be mistaken in one place or the other; for, if 24 require 260, 1,200 will require 13,000. Perhaps instead of 260 it should read 60, or rather 65, or 96 bushels instead of 24.

[29] M. Tessier says, page 731: "All being well calculated, one is able, in the

preliminary cleansing, which is indispensable when the wheat is visibly contaminated, and the simplest method for which, as we have seen (§183), cost 12 sous per hectoliter in 1790; nor take into account the cost of the fuel used to heat the water as is prescribed, or even to boil it,[30] sometimes with several repetitions.

The surest way of using these limings is to let the wheat soak for 24 or even 48 hours. In the third method, the pigeon or fowl droppings, which furnish the volatile alkali, must soak for 15 days in water, and this preparation has the disadvantage of having a very bad odor.

In order that a preventive may be generally adopted, it must be extremely simple and inexpensive. One can scarcely regard as combining these advantages treatments in which a large quantity of boiling water must be used, which are so precarious in their effectiveness that they act certainly only by means of long or expensive preliminary cleansings, such as washing in several waters or oft-repeated siftings, and that require the wheat to remain in them for 24 or 48 hours. The method I propose, copper sulphatation, requires nothing else, even for the most contaminated seed, except that the seed soak for a half hour in a solution that is easily prepared cold. Moreover, the manner in which this is done is not important. I require, furthermore, it is true, that this seed be freed of whole bunted kernels, or that they be removed during the operation; but that can be done by extremely simple means that are at the disposal of all farmers.

191. However, M. Tessier's preventive methods, which were without doubt the surest and most convenient of the time in which he wrote, should be studied in his own works. On the basis of what he himself says of them, they should be compared

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vicinity of Paris, to lime 12 bushels of wheat for 8 sous, employing the most expensive method, that in which the lime alone is dissolved in the water."

But the most expensive liming, it seems to me, is the one the price of which is given, page 730, at 107 francs 10 sous, the 1,200 bushels, Parisian measure, or 21 sous 6 deniers per 12 bushels and in which the lime is combined with fixed alkali.

[30] I prescribe, it is true, dissolving the sulphate in a little boiling water when one is in a hurry. But this may be dispensed with, and the solution will be retarded only three or four hours.

with sulphatation, which, I think, is infallible, much simpler, and less expensive.

192. I have been led to this by the discovery of the direct cause of the disease, which I have followed with care and described in great detail. The new method that I have found of studying internal plants, or rather, these amphibious species whose development begins in the open air and finishes within the body of other plants, has led me to some observations that may be of interest in natural history, and has given me the occasion to point out some errors concerning the nature of these microscopic organisms.

The success unfailingly obtained by those who have tried or will carefully try the copper sulphate treatment should cause its general adoption. But, even if it were practiced by only a small number of careful farmers, the wheat that they would harvest, being absolutely bunt-free, might, without need of any treatment, serve to propagate pure seed, and to extirpate the germ of the infection from the soil of the Empire.

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EXPLANATION OF FIGURES

Plate 1

Fig. 1. Fully mature, dry bunt globules.
Fig. 2. Globules that have soaked for some time in water; some appear to be attached to filaments, with which they form a sort of cluster.
Fig. 3. Bunt globule or gemma, beginning to put forth a bud.
Fig. 4. This bud becomes a small stalk, which is elongated in figs. 5, 6, 7, 8, 9.
Figs. 10–11. Stalks forked or bearing an antler-like structure.
Figs. 12–21. The origin and formation of the aigrette.
Figs. 22–25. Aigrettes that have fallen into the water, after having begun to form in the air.
Fig. 26. Aigrettes seen by reflected light and less magnified.
Fig. 27. Tuft-like or stupose stalk.
Figs. 28–32. Plumose stalks bearing fruits of some kind.
Fig. 33. Early development of a tuft-like or stupose stalk.
Fig. 34. Another mode of development of stupose stalks.
Fig. 35. Late development of an aigrette.
Fig. 36. Empty globule, much enlarged, after putting forth a long stalk.
Figs. 37–38. Last observable developments of the shoots or secondary stalks that make up the stupose stalks.

Plate 2

Figs. 1–8. Much enlarged pistils and stamens of a very young bunted head.
Fig. 9. A much more advanced flower.
Fig. 10. Flower of a healthy head ready to appear.
Figs. 11–14. Bunt globules, such as are found in very slightly advanced embryos.
Figs. 15–18. More nearly mature globules.
Figs. 19–20. Globules that seem to be implanted upon a solid substance.
Fig. 21. Globules or gemmae that are beginning to become opaque.

Plate 3

Fig. 1. Globules such as are often seen in the embryo of healthy wheat. They appear to be flattened, and are very different from those represented in Plate 2.

Fig. 2. Globule that is sometimes found with the eel vibrio and other vibrios in kernels of wheat that have produced stalks several inches high.

Fig. 3. Bunted head with elongated, hollow kernels. Similar heads without bunt are sometimes found, the kernels of which are empty.

Fig. 4. Seeds from the same head, detached and slightly enlarged.

Fig. 5. A bunt stalk of unusual shape, developed from gemmae found in the kernels of the preceding figure.

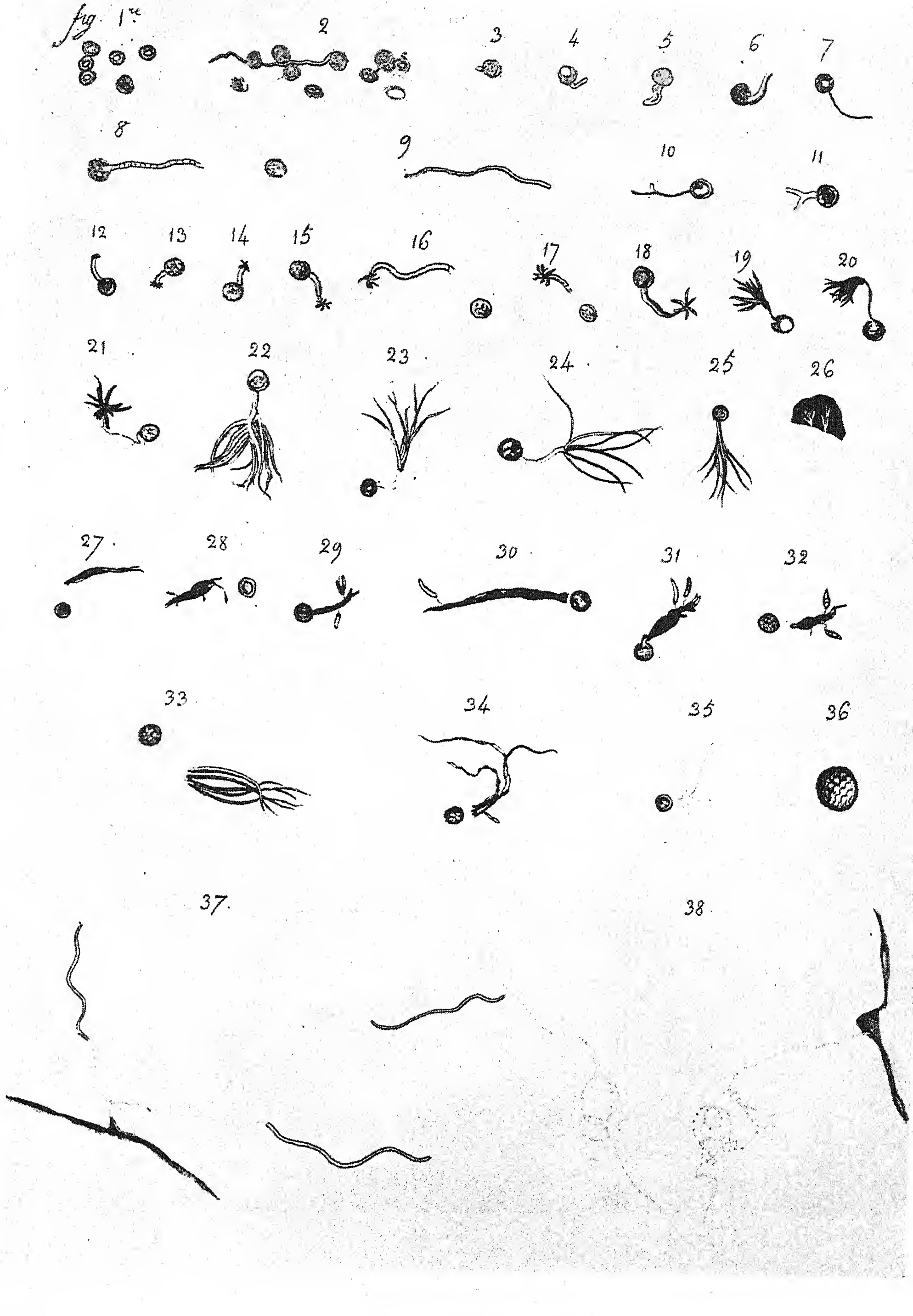
Figs. 6–7. Extremely young initial of a smutted or blighted head, enlarged with the magnifying glass and the microscope.

Fig. 8. Much enlarged clusters of globules or puccinias of smut or blight, found in similar but more advanced heads.

Fig. 9. Globules that become detached from ergot of rye or other gramineous plants, and, at a slightly raised temperature, become active and put forth stalks.

PLATES

Pl.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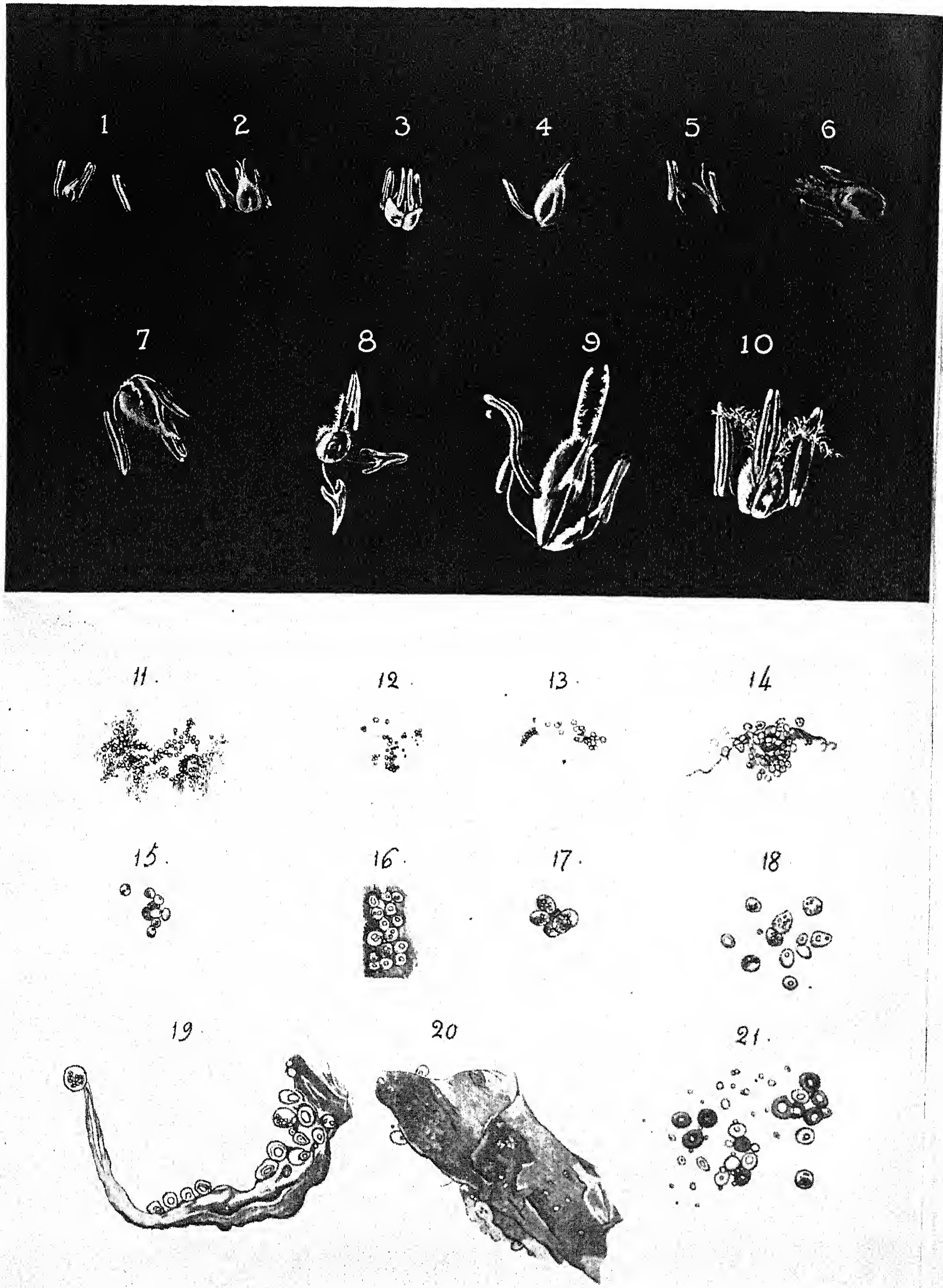
Pl. 2



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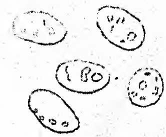

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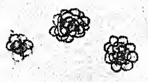

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